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Practical and Theoretical Mechanics and Chemistry.

On Hydraulic and Common Mortars. By General TREUSSART, *Inspecteur du Génie. Translated from the French by J. G. Totten, Lt. Col. of Eng. and Brevet Col. United States Army.*

ART. III. *Experiments on several hydraulic limes of the environs of Strasburg, on the Metz lime, and on Boulogne pebbles.*

When I was sent to Strasburg in 1816, they were in the habit, there, of using fat lime only. One of the two dikes enclosing the navigable canal which passes from the town to the Rhine across the ditch of the place, requiring repairs, I had occasion to remark that while the two facings, which were of cut stone, were much disjointed, the interior of the dike, which was a mass of concrete, was in good condition, and not a drop of water passed through it. The concrete being very hard, and, as it seemed, not made of brick, or tile dust, I conjectured that it had been composed of hydraulic lime, and I made researches accordingly. I learned that the Millers along the Bruche had used, for a long time, in the repairs of their works, a particular species of lime, which they obtained from a village, at the foot of the Vosges, called Obernai.* Mr. Mossère, Engineer of roads and bridges, told me that he had used this lime in the works of the canal of "*Monsieur*," and that he was well satisfied with it; and, on making the essay, I found it

* This village is situated between Schelestadt and Strasburg: upon the map of Cassini it is written Ober-Ehnheim, but it is pronounced Obernai, and is so written on some maps.

to be eminently hydraulic: it appeared to me to be in no respect inferior to the Metz lime, which I had seen employed with success, at that place, in 1800 and 1801. At other stations, I had several times made hydraulic mortars of fat lime and brick, or tile, dust. At the great works of Vésel, where I was employed three years, considerable use was made of trass, which was brought from Andernach by the Rhine; and on the experience I had acquired of hydraulic limes, I introduced the use of the Obernai lime in all the constructions of the works of Strasburg, both in and out of water. I have already observed that all the revetments comprised between *porte de Pierre* and *pont Royal*, giving a development of about 1650 yards, were made with hydraulic lime. Later researches have shown me that hydraulic lime is to be found at the foot of the Vosges, from Belfort to Vissembourg. I shall give experiments which I made with hydraulic lime from Altkirch, Obernai, Rouxviller, Ingviller, Oberbronn, Verdt, &c.

There are no certain means of knowing by inspection whether a lime-stone will give fat lime or hydraulic lime, for there are hydraulic lime-stones of several colours: those of Alsace are generally of a slate blue, like those of Metz, but those of Altkirch, and the hydraulic chalk of Vitry are white; the Boulogne pebbles, and the English lime-stone of which Parker's cement is made, are red. I may say, however, that when a lime-stone is blue, it is a presumption that it will afford hydraulic lime. It is remarkable that in lime-stone giving fat lime, the iron exists as red oxide, while it exists in the lowest state of oxidation in a great proportion of the hydraulic lime-stones, even when left for a long time exposed to the air; the same thing happens with good slate, and it seems to me to be owing to the presence of clay in the hydraulic lime-stones and in the slate, with which the iron is probably in combination. As I have before said, the Boulogne pebbles, and Parker's cement stone, were exceptions; but this might be due to some peculiar cause: the sea-water might have some influence on the oxidation of the iron contained in the Boulogne pebbles.

All the blue hydraulic lime-stones which I have caused to be calcined, gave a yellow ochre colour whenever the heat was not great. If the calcination was raised, the colour passed, successively, to fawn, ash-gray, and, finally, if the heat had been very great, to slate blue. I cannot satisfy myself as to the cause of the blue colour which the lime-stone takes by a high calcination, because it seems necessary to suppose that this effect is the bringing back of the iron to its first degree of oxidation, which is possible, but does not seem to me easy to explain.

A like effect appears to take place in the calcination of clays that contain oxide of iron. Some clays submitted to calcination are blackish, because of the iron being present at the lowest state of oxidation: at a certain degree of heat, the iron passes to the state of red oxide, and the clay takes the decided colour commonly seen in bricks: if the heat be augmented, the red colour weakens, and becomes fawn or straw colour; if the heat be pushed to a high degree, the clay becomes of an ash colour and afterwards of a very decided slate-blue colour—the iron having then lost a great part of its oxygen. If slate, and the clay just described, be calcined at the same time, the iron will pass with much more difficulty to the state of red oxide in the slate than in the clay. The degree of heat necessary to bring the iron in the slate to the condition of red oxide, will have caused the iron in the clay to pass through all the stages of oxidation before mentioned, and have imparted the blue colour to the clay. Are these different phenomena due to this, that while, in the slate, the iron is combined with the alumine, there

is no such combination in this clay? In clays which are strongly calcined, has the alumine the property of abstracting the oxygen from the iron? It is for chemistry to clear up these points.

M. Berthier has given the following method of detecting hydraulic limestones. "The stone must be pulverized and sifted through a silk sieve: 10 grammes of the powder being put in a capsule, muriatic acid, diluted with a little water, must be poured on, little by little. (In want of muriatic acid, nitric acid, or even vinegar, may be used.) The whole should be constantly stirred with a glass or wooden rod, as long as there may be any effervescence; this having ceased, the solution must be evaporated, in a gentle heat, to the consistence of paste; about one pint of water must be added and the whole filtered; the clay will remain upon the filter; it must be dried in the sun or before the fire, and weighed; or, still better, it must be raised to a red heat in an earthen, or in a metallic crucible before being weighed; limpid lime-water must then be thrown into the solution, as long as it forms a precipitate therein; this precipitate should be separated, as soon as possible, by the filter; it is magnesia (sometimes mixed with iron and manganese;) it should be washed with pure water, and dried at a high heat before taking its weight." The weight of the clay retained by the filter, compared with the weight of the lime-stone, will indicate whether it be hydraulic limestone or not. But it might happen that the substance remaining on the filter would be all sand; this may be determined before drying; for if it be sand only, it will be rough to the touch, while if it form a soft and ductile paste, it will be clay, that is to say, a mixture of alumine and silex. Besides, by drying at a high heat, it will be seen if a compact mass be obtained, which will be the case with clay highly heated, or if it be pulverulent matter, which will show that little or no alumine is present. As to the rest, we do not yet know, accurately, in what proportions the silex and alumine ought to be present to constitute the best hydraulic cement. The best manner of ascertaining whether a lime is hydraulic, is the following: take quick lime as it comes, properly calcined, from the kiln; reduce it, with water, to a thick paste, and place enough of it on the bottom of a tumbler to fill it for one-third or one-half of its height; three or four hours after, fill the tumbler with water, and leave all at rest; after two or three days, touch the lime lightly with the finger, to ascertain whether it begins to harden: if it be very hydraulic, it will have taken, after eight or ten days, such a consistence, that no impression can be made on the lime by pressing strongly with the finger. We should assure ourselves whether there has been, in fact, no impression, by throwing off the water and washing the surface of the lime, which will be covered with a thin layer of lime softened by the immediate contact of the water. If the above result be obtained only at the end of twenty, thirty, or forty days, the lime should be regarded as only feebly hydraulic, and if there be no consistence in the lime after the lapse of about forty days, it cannot be regarded as at all hydraulic. This process, which is very simple, is that which I always follow—it was indicated by Mr. Vicat.

Before giving my first experiments, I will explain the processes I followed, both in making mortars, and in breaking them in order to determine their tenacity. In my first experiments, I fixed the proportions of my mortars, by slaking the lime to dry powder with one-fifth of its volume of water, and measuring this lime in powder. Afterwards I measured the lime in paste, in order to approach the mode ordinarily pursued in practice with fat lime. I shall take care to state in every instance, which of these modes

of measurement I followed. When I had united the lime to its proper proportion of sand, or other substance, I mixed them well together, adding water, till the consistence was like honey; and I passed the mortar seven or eight times under the trowel. The mortar being made, I put it in wooden boxes which were six inches long by three inches wide, and three inches deep, leaving them in the air for twelve hours, so that the mortars might be somewhat stiffened. They were then placed in a cellar, within a large tub filled with water. I examined the mortars from time to time, and noted the number of days required to harden. I called the mortars hard, when, on pressing them strongly with the thumb, no impression was made on the surface. All the mortars were left in the water one year; at the end of which time they were withdrawn, and I scraped off the four sides or faces with the chisel of a stone cutter until nearly half an inch was removed, when they were rubbed upon a stone until they were reduced to parallelopepids of 6 inches long by 2 inches square. By means of a wooden form which they were made to fit, all were reduced, very exactly, to the same dimensions, and had the four faces well squared. It will be observed that I took off from each side about half an inch, with the view of submitting to rupture only the portion which had not been in contact with water. I ought to notice that in doing this it was often found that the mortars were harder at the surface than in the interior: sometimes the contrary happened. By taking off a portion ($\frac{1}{2}$ inch) from each face, I rejected all, that, from any cause, had received a different degree of hardness from the interior.

To ascertain the tenacity of mortars, the following process was followed. I adapted two pendant, iron, stirrups to a horizontal beam (see plate I. fig. 1.) They were placed parallel to each other and distant four inches in the clear—the lower parts being exactly level. On these stirrups, the parallelopepid of mortar which was to be tested, was placed, passing it through a rectangular collar of iron, a little larger than the prism of mortar, and terminated below by a hook. This collar exercised its pressure upon the mortar by its upper horizontal bar, of which the transverse section had the form of a rounded wedge; and being brought against a bracket, or check, it was midway between the stirrups. To the hook of the collar was suspended a common scale pan, that was loaded by adding successive weights until the prism of mortar broke—which it did with noise. The weights were then summed up, adding, always, 22 lbs. for the weight of the scale pan, cords and collar. In the beginning, as a weight, I poured sand into a box placed on the scale pan, but I renounced this method, because it was too slow. I perceived that it was important to break the mortar promptly; it having several times happened that, when the mortar had supported for some time a weight approaching that which should break it, it would give way, after having taken off a weight of 11 lbs. in order to substitute a greater. On cutting the parallelopepids down to the proper size, I judged nearly of the weight they would sustain; and a weight approaching this was put on at once; after which small weights were added, one after the other, until the mortar broke. When the mortars were capable of sustaining a great weight, I added weights by 10 pounds at a time, because this is a small quantity compared with the total weight, and it is important that the fracture be made promptly. The above are the means I used, and they seem to me to be preferable to any that I have seen described by authors. Some have directed that the mortars should be loaded with weights, until crushed; but the moment when they begin to yield is difficult to judge of, as the angles often break before the middle, and it is not clearly seen when the substance under trial has really yielded to the load. Others

have directed that the body of which the strength is to be ascertained, be placed at the end of a strong table to which it is to be fastened; a scale pan is then to be suspended from a portion projecting a certain distance from the table, and weights are to be added till rupture occurs; but it is to be feared that the mortars, or the stones, submitted to this trial, will sometimes project a little more, and sometimes a little less; the weights acting thus at the extremity of unequal arms of levers, will give different results for substances that should exhibit the same tenacity: it is also to be feard that fastening the mortars more or less stifly will influence their resistance.

Mr. Vicat, to judge of the resistance of mortars, employed the following method—reported in pp. 34 and 35 of his memoir: he lets fall upon the surface of the mortar, from a height of 2 inches, a rod of steel slightly conical, and terminated at its end by a plane surface 0.066 inches in diameter; this rod, loaded with the weight of $2\frac{2}{10}$ lbs., penetrates a certain distance into the mortar, giving, thereby, the relative hardness.

As it is the resistance that mortars oppose to steady or unimpulsive forces that it is important to know, he thought he might conclude, from some experiments that he reports, that "*the squares of the numbers which express the penetration of the rod are reciprocally proportional to the resistances to the force which tends to break the mortars.*" On this principle he has transformed, by calculation, the penetration of the rod, into numbers proportional to the resistances. I must make, however, on this subject, the following observations.

1st. It is difficult to appreciate exactly the penetration of the rod: 2d. It does not appear to me to be proved, by the few experiments given by Mr. Vicat, that the squares of the numbers expressive of the penetration are always reciprocally proportional to the resistances; 3d. If the rod fall on a grain of gravel, or on a large grain of sand, or even on a grain of lime, we should be liable to infer, from the penetration, conclusions altogether inaccurate as to the resistances of the mortars; 4th. The rod is permitted to fall on the surface of the mortar, and it often happens, as before observed, that the hardness of the surface differs much from that of the interior.

All these causes of error united have conducted Mr. Vicat to conclusions which are sometimes entirely opposite to results obtained in my experiments; I shall take pains to point these out as they present themselves. It will be noticed that to determine the resistance of mortars, I have no calculations to make; by the means I employed, these determinations were perfectly independent thereof, and I have only to enter in the table the number of pounds the mortars are loaded with at the moment of rupture.*

I am now about to present the first results of the experiments made with Obernai lime, and with other hydraulic limes of the environs of Strasburg. The Obernai lime being that which was most often used at the works, I sent

* Since the first part of this memoir was written I have seen a new memoir of Mr. Vicat's in which he says, page 117, "We thought ourselves entitled to conclude from a certain number of experiments described in our first work, that the squares of the numbers expressing the penetration of the rod, falling with a given weight, are reciprocally proportional to the relative or absolute resistances of the substance; and, on this principle, the penetrations were transformed by calculation into numbers proportional to these resistances. But, in accordance with the very judicious observations of Mr. Vauthier, Engineer, we have decided to return to the numbers which express the penetration."

I will again observe that it is the tenacity of mortars submitted to unimpulsive or steady forces which it is important to know, and that this will not be obtained by numbers which express the penetration of a rod submitted to an impulsive force.—Av.

a specimen to Mr. Berthier, who analysed it, with the following results: Lime, 0.422; Magnesia and iron, 0.050; Silex, 0.105; Alumine, 0.043; Carbonic acid and water, 0.380. This lime-stone differs but little from that of Metz, which contains as follows: Lime, 0.445; Magnesia and Iron, 0.067; Silex, 0.053; Alumine, 0.013; Carbonic acid and Water, 0.412; loss 0.010.

Table II.

No. of the mortar.	Composition of mortars.	No. of days required to harden in water.	Weights which they supported before breaking.
	Common bricks of Strasburg		462 lbs.
	Refractory bricks of Sufflenheim		572
1	Yellow Obernai lime alone in paste,*	8	372
2	do. do. do. slaked to powder and measured in powder† 1 ; Common sand 1½ ; 2½	10	224
3	Lime the same 1 ; Sand the same 2 ; 3	12	253
4	Lime the same 1 ; Sand the same 2½ ; 3½	12	176
5	Lime the same 1 ; Sand the same 3 ; 4	14	92
6	Lime the same 1 ; Sand the same 1 ; Trass 1 ; 3	4	429
7	Lime the same 1 ; Trass 2 ; 3	4	299
8	Gray Obernai lime alone, in paste	8	354
9	do. do. do. slaked to powder and measured in powder 1 ; Common sand 1½ ; 2½	10	400
10	Lime the same 1 ; Sand the same 2 ; 3	10	422
11	Lime the same 1 ; Sand the same 2½ ; 3½	12	187
12	Lime the same 1 ; Sand the same 3 ; 4	15	106
13	Lime the same 1 ; Sand the same 1 ; Trass 1 ; 3	4	473
14	Lime the same 1 ; Trass 2 ; 3	4	328
15	Blue Obernai lime alone, in paste	20	275
16	do. do. do. slaked to powder and measured in powder 1 ; Common sand 1½ ; 2½	14	136
17	Lime the same 1 ; Sand the same 2 ; 3	15	154
18	Lime the same 1 ; Sand the same 2½ ; 3½	16	117
19	Lime the same 1 ; Sand the same 3 ; 4	18	79
20	Lime the same 1 ; Sand the same 1 ; Trass 1 ; 3	5	378
21	Lime the same 1 ; Trass 2 ; 3	5	339

* The lime of the hydrates No. 1, 8, and 15 is not of the same burning as the lime of the mortars of the Table. Au.

† I must premise that by the expression, *slaked to powder*, often used in the Tables,

Observations on the experiments of Table No. II.

To make the above mortars I took lime as it came fresh from the kiln, and slaked it to dry powder by throwing on one-fifth of its bulk of water; I left it in this state for twelve hours, and then, having measured the powder, I added the quantity of water necessary to reduce it to paste. I afterwards added to the lime the several quantities of sand and of trass indicated in the table, and I, in all cases, mixed the constituents until a homogeneous mortar was obtained, which required that it should pass six or eight times under the trowel. The mortars were made of the consistence of honey; and were put in the small wooden boxes before mentioned; being lightly compressed with the trowel and the hand, they were left in the air during twelve hours: they had then somewhat stiffened, and in this state were placed in a cellar, in a large tub filled with water: I took care to examine them from time to time, and to note the number of days they required to harden to such a degree that on being strongly pressed with the thumb, no impression was made. At the end of a year they were withdrawn from the water and were broken in the manner described, page 304. In order to institute a comparison between the resistance of my mortars and the building materials of the country, I broke, in the same manner, parallelopepids of bricks, having the same dimensions: the average of the common brick of the neighbourhood of Strasburg, gave me 462 lbs., as is expressed in the table; but the refractory bricks of Sufflenheim, are much stronger, and gave 572 lbs.

As I have before remarked, the Obernai lime is yellow when not fully calcined; when a little more burned it is of a dirty yellow; afterwards it passes to ash-gray; and at last, when too much burned, it is of a slate blue, and pieces of vitrified lime are often found. The lime being burned with wood, it is difficult to obtain a uniform calcination: there are always some pieces of lime too much, and others too little burned. Care was taken to reject the pieces of both extremes; but the mortar made at the works always contained, notwithstanding, lime of the three colours mentioned. If the lime-burners would burn their lime for a little longer time, making a less intense fire, they would not consume more wood and would give better results. But it is difficult to change usages.

The object of the experiments of Table No. II. was to know the degree of calcination which is best, and the quantity of sand most proper to mix with the lime. In table No. 1 where I operated with several kinds of lime, I slaked them to dry powder with a bulk of water equal to half the bulk of lime; but the Obernai lime is well reduced to powder with a fifth of its volume of water. With this quantity there is obtained, moreover, nearly the same volume of lime in powder, as when slaked with the half of its volume of water. In fact, 0.005 of yellow Obernai lime reduced to powder and slaked with one-fifth of its volume of water, gave 0.0105 of lime in powder, and, consequently, 1 part would give 2.10, a result differing little from that of table No. 1.

Table No. II. shows that lime-stone which had been calcined so as to yield a lime slightly gray, was that which gave the best result; that which

must be understood, lime which was slaked as it came from the kiln, with only a small quantity of water, to reduce it to dry powder. As to lime reduced to dry powder by being left to spontaneous slaking in the air, it is called *lime slaked in the air*, or *air-slaked lime*. I sometimes use in the tables the sign + signifying *more*, and the sign — signifying *less*.—*Author*.

was blue was slow to slake; when reduced to paste it formed with sand and with trass, a mortar susceptible of swelling considerably, because of its preserving, even in this state, the property of absorbing water for a long time. All the boxes containing mortar made of blue lime were totally disjointed by the enlargement of the bulk of mortar. I was not, at first, aware of this effect, and having caused a coat of rough-cast, composed of Obernai lime and sand, to be applied to one of the Government buildings, I was surprised, after five or six months, at seeing, on many points of the surface, swellings of the size, nearly, of a half dollar; these portions finally fell off, and it was then seen that they had been caused by particles of blue lime, about the size of a pea, that were beneath. These particles had not had time to slake thoroughly; and, as imperfectly slaked lime has a strong avidity for water, they absorbed water from the air, and increased in bulk. The force must have been great to break the very hard mortar which covered these particles. This rough-cast has now been on six years; it is very hard, and has perfectly resisted all inclemencies of the seasons. The property which hydraulic lime, too much calcined, has, of swelling considerably after being made into mortar, might be availed of, under certain circumstances. If it were required, for example, to fill cracks in old walls where the hand could not be introduced, or to fill spaces washed from under old foundations, mortar made of highly calcined hydraulic lime might be advantageously used, because the mortar by swelling would more perfectly fill the cavities. But especial care should be taken not to use it in new constructions, as it might cause serious accidents. It is stated that two locks having been made, a short time since, with lime that had not been sufficiently slaked, the mortar swelled to such a degree that all the cut stone was displaced, and it was necessary to rebuild the masonry. This accident happened to the works for the improvement of the navigation of the Vésère.

Table No. II. teaches that the yellow lime and the blue lime gave, alone, greater resistances than when mixed with sand. But I ought to observe that no conclusion should be drawn from this, as the three experiments on hydrates were made with lime different from that used in making the mortars of the table. At the time of making these experiments, I was far from thinking there could be so great a difference between mortars made of pieces of lime which seemed to have sustained the same degree of calcination. The best results with the Obernai lime alone, are 372 lbs. for the yellow, 354 for the gray, and 275 lbs. for the blue. I regret not being prepared for this superiority of the hydrate over mortars, but I was far from expecting it: otherwise, after having made the experiments with the limes alone, I should have made mortars, by adding successively, $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, &c. of sand, so as to judge better of the effect of sand; but it was only on breaking the mortars at the end of a year, that I could know the effect, and I have often, on such occasions, obtained results that surprised me, and been conducted to new experiments, requiring another year to furnish results.

It is seen that after a certain quantity of sand has been added, the mortars lose much of their tenacity: and that for every degree of calcination, trass considerably augments the resistance. It is remarkable that mortars made of lime, sand, and trass, are in general better than those made with lime and trass, without sand: I have, however, found some exceptions. Mr. Vicat thinks that puzzalonas only slightly energetic, suit best with hydraulic limes; but my experiments do not support this opinion; as the

trass I used was very energetic, and it will be seen that I always had very good results on mixing it with very hydraulic lime. I obtained, in the above table, better results with Obernai lime, trass, and sand, than with Obernai lime and trass without sand, and it will be seen, by experiments which follow, that I found the like results with fat lime. In the 7th number of the *Memorial de l'Officier du génie*, I have advanced that, in important constructions, it is not prudent to employ hydraulic lime without a little trass. Mr. Vicat is of a different opinion: for we see a note in the *Annales des Mines*, Vol. X., page 501, wherein he says that the success which has attended the use of hydraulic lime in different works which he cites, ought to suffice to undeceive those who partake my fears as to the insufficiency of hydraulic lime without trass, in important constructions. I have, nevertheless, had an opportunity of seeing the concretes used at the canal *Saint-Martin*, at Paris, and at the basin of the *Palais Royal*, made of the hydraulic lime manufactured by Mr. Saint-Léger, and they are much less hard than those made at Strasburg. I do not doubt, however, that the concrete employed at the *Saint-Martin* canal will well fulfil its object. Being obliged to cover all the bottom of this canal, which has an extent of about 4400 yards, with concrete, in order to keep the water from filtering into the cellars of the inhabitants, it would have much augmented the expense, to have added to this mortar, cements analogous to trass. Concrete composed of Obernai lime and sand, without trass, was often used at Strasburg, for the foundations of revetments, of piers of bridges, &c.; but by "important constructions" I meant Locks and Dams, which have a great pressure of water to sustain, perhaps for a long time. There is no doubt that if such a work were executed with gray lime like that of the preceding table, there would be nothing to fear: but this would require the rejection of a great part of the kiln, and would make the mortar very dear: the extremes, only, can be rejected, so that the lime used is a mixture of the three qualities. If a mean be taken of the three kinds of mortar found under Nos. 3, 10, and 17, we shall have 276 lbs. for the result, while the mean term of the three Nos. 6, 13, and 20, is 427 lbs. I have often, however, had results much weaker than those of the above table, as will be seen in the sequel. I have even had occasion to remark that hydraulic limes coming from the same quarry, and appearing to be calcined nearly to the same degree, have, nevertheless, given very different results: in these cases, the lime, which, mixed with sand had given me a weak resistance, had always afforded a very good mortar, when I used sand and trass with the lime. I am convinced that these last mortars have given, on an average, a resistance more than double the first; I persist, therefore, in thinking that in important constructions, such as Locks and Dikes, which have constantly a strong pressure of water to support, it is prudent to put a little trass, or some analogous substance, into the mortar, even when the hydraulic lime used is as good as the very good hydraulic lime of Strasburg. It is for the Engineer who has the work to execute, to examine the quality of the lime he is to employ, in order to fix the quantity of these matters to be added in the mortar. In my opinion a Government should not regard a light additional expense, when the object is to obtain constructions that will last a long time without needing repairs: unfortunately the contrary system, which I think a very bad one, most commonly prevails.

I ought to premise that in the experiments which I shall report, I shall abstain from conclusions too general. With the limes of Alsace I have often noticed contradictory results, and even, as before mentioned, with

limes from the same quarry. We have not, as yet, collected enough facts to establish a general theory: that which may be true as to the limes of one country, may not be true as to those of another. I give the results which I have obtained. It is very desirable that Engineers who have some leisure, should make experiments; as it is only when a great many results shall have been collected that we shall be able to deduce any general principles: those which I shall present must be considered as belonging only to the limes which I shall use.

I now proceed to report other essays upon other limes of the environs of Strasburg.

Table III.

No. of the mortar.	Composition of the mortars.			No. of days which they took to harden in wat'r	Weight which they supported before breaking.
1	Altkirch lime alone, in paste			10	268 lbs.
2	do. do. slaked to powder and measured in powder	1	3	12	174
	Common sand	2			
3	Lime the same	1	3	4	539
	Sand the same	1			
	Trass	1			
4	Lime the same	1	3	4	535
	Trass	2			
5	1st Villé lime alone, in paste			25	119
6	Lime the same, slaked to powder and measured in powder	1	3	28	68
	Sand	2			
7	Lime the same	1	3	4	473
	Sand	1			
	Trass	1			
8	Lime the same	1	3	4	473
	Trass	2			
9	2d Villé lime alone, in paste			25	110
10	Lime the same, slaked to powder and measured in powder	1	3	28	114
	Sand	2			
11	Lime the same	1	3	4	396
	Sand	1			
	Trass	1			
12	Lime the same	1	3	4	475
	Trass	2			
13	Rosheim lime alone, in paste			6	484
14	Rosheim lime, slaked to powder and measured in powder	1	3	14	209
	Sand	2			
15	Lime the same	1	3	4	462
	Sand	1			
	Trass	1			
16	Hochfeld lime alone, in paste			12	207
17	do. do. slaked to powder and measured in powder	1	3	22	136
	Sand	2			
18	Lime the same	1	3	4	429
	Sand	1			
	Trass	1			

Table III.—(continued.)

No. of the mortars	Composition of the mortars.			No. of days which they took to harden in wat'r	Weight which they supported before breaking.
19	Verdt lime alone, in paste			8	484
20	do. do. slaked to powder and measured in powder	1	3	10	279
	Sand	2			
21	Lime the same	1	3	4	510
	Sand	1			
	Trass	1			
22	Lime the same	1	3	4	310
	Trass	2			
23	Oberbron lime alone, in paste			10	264
24	Same lime measured in paste	1	3	10	220
	Sand	2			
25	Same lime do.	1	3½	8	352
	Sand	2½			
26	Yellow Bouxviller alone, in paste			10	299
27	Gray do. do. do.			30	99
28	Blue do. do. do.			40	79
29	Yellow do. measured in paste	1	3	10	286
	Sand	2			
30	Lime the same	1	3¼	10	187
	Sand	2¼			
31	Lime the same	1	3½	10	165
	Sand	2½			
32	Lime the same	1	3¾	12	132
	Sand	2¾			
33	Lime the same	1	4	12	99
	Sand	3			
34	Gray Bouxviller lime, measured in paste	1	3	25	77
	Sand	2			
35	Blue do. do. do.	1	3	30	66
	Sand	2			
36	Yellow do. do. do.	1	3	5	407
	Sand	1			
	Trass	1			
37	Metz lime alone, in paste			15	169
38	Metz lime slaked to powder and measured in powder	1	2½	16	312
	Sand	1½			
39	Lime the same	1	3	16	262
	Sand	2			
40	Lime the same	1	3½	16	176
	Sand	2½			
41	Lime the same	1	4	18	143
	Sand	3			
42	Lime the same	1	3	4	466
	Sand	1			
	Trass	1			
43	Lime the same	1	3	4	262
	Trass	2			
44	Lime the same	1	4	4	385
	Trass	3			

Observations on the Experiments of Table No. III.

The Altkirch lime comes from a lime-stone which is grayish: all the others from stones that are blue like those of Obernai and Metz. All were slaked to powder with one-fifth their volume of water, and left in that state for twenty-four hours, before making them into mortars. Several being too remote to be brought into use at the works, I contented myself with some experiments on their hydraulic properties. It is seen that the Altkirch lime is good. The piece of lime brought me had, probably, been out of the kiln a number of days, which should considerably influence the result, as will be soon seen. It is possible, therefore, that this lime is better than the experiments indicate. Two different kinds were brought from Villé. I was told that it was as good as the Obernai lime. The first kind is brown, and the second grayish: we see that the resistances are feeble. The limes of Rosheim, Hochfeld, Verdt, Oberbronn, and Bouxviller gave very good results whether alone or in mortar. It will be seen, also, that with the limes slaked to powder and measured in that state, the mortars consisting of lime and sand, generally supported less weight than the lime alone. It is possible that there was too much sand in them. I began with the principle, admitted by several Engineers, that it is better to err by excess of sand than by excess of lime; but it appears this is not always true, for the experiments of tables Nos. II. and III. show that when the mortars had a little too much sand, their resistance was much diminished. It is possible also that some hydraulic limes may become harder when alone, than when mixed with any proportion of sand whatever.

The Oberbronn lime was measured in paste; it appears that with two and a half parts of sand, it gave a better result than with two parts only, and that this result was superior to that obtained with the lime alone.

As the Bouxviller lime might be used at Strasburg I treated it with more detail. We see that this lime loses much by being too much burned, and that it cannot support as much sand as that of Oberbronn; and, also, that with all these limes, the mortars were much improved by adding trass to the lime and sand. It is an error, therefore, to suppose that energetic puzzalonas are not adapted to good hydraulic limes. Tables Nos. II. and III. show that different limes take very different proportions of sand. Thus, as before observed, various experiments induce me to believe that pieces of lime from the same quarry may give very different results, although calcined to the same degree. It also appears that different degrees of burning require different proportions of sand; it becomes, therefore, difficult to fix accurately the proportion of sand which should enter the mortar.

The above experiments were a first essay only: I proposed making others, with a mixture of lime calcined to different degrees, and to repeat the experiments on several burnings, so as to be able to take a mean. At Strasburg we adopted one part of lime to two and a half parts of sand. The mortar was good, but it, possibly, was not the best that could be made. Supposing the best proportions, as to hardness, were one part of quick lime to two parts of sand, it would still remain to determine whether these proportions should be adopted; because, if in putting a little more sand the result should be but slightly inferior, it might be done unhesitatingly, if the economy were sensible. I was about to make these experiments on the different limes of the environs of Strasburg, when I was obliged to quit the place, in consequence of the new functions to which I was called.

Towards the end of the table, are the experiments made with Metz lime.

It appears that one part of lime in powder, and one and a half of sand, is the mixture which gave the best result, and that it is stronger than the lime alone. Notwithstanding the mortars were made in the month of June, the hardening was slow; and experience teaches that mortars harden sooner in this season, than in winter. The comparisons of No. 42, 43 and 44, show that for the hydraulic limes of Alsace, better results are obtained with sand and trass, than with trass alone. No. 44 which was made with one part of lime in powder, shows that this lime will bear a good deal of trass without losing much of its resistance.

In the above table, the limes used alone, and in the mortars, came from the same piece of limestone, in each case.

I shall conclude my observations on Table No. III, by saying that in the course of my experiments, I have had occasion to observe, more than once, that discordant results were obtained when I used lime that had been slaked for several days. When I made the experiments with the Oberbronn lime, Nos. 23, 24 and 25, I put aside a portion of the lime in powder which I had used. At the end of 15 days, I repeated the experiments recorded at Nos. 23 and 25: for No. 23, which was lime alone, the hardening required 30 days instead of 10 days, which were necessary, when the fresh lime was used. At the end of the year, the piece of hydraulic lime, broke with a weight of 154 lbs, while No. 23, which was made immediately, supported, before breaking, a weight of 264 lbs. As to the mortar made of one part of lime, measured in paste, and two and a half parts of sand, the same as mortar No. 25, the hardening required 25 days instead of 8 days, and the mortar broke with a weight of 154 lbs, while that of No. 25, made immediately, supported 352 lbs, before breaking. I put this lime aside for fifteen days, to see if an old saying of masons, that lime loses its energy in the air, had any foundation. The experiment convinces me that the saying is true, at least as regards the hydraulic limes that I treated; but I wished to see, also, what results I should obtain, with mortars made in the same manner and of the same piece of lime, but at different times. I made, in consequence, the following experiments.

Table No. IV.

No. of the series.	Composition of the mortars.	Made immediately.		After six weeks.		After 2½ months	
		hardening.	w't. supported	hardening.	w't. supported	hardening.	w't. supported
		days	lbs	days	lbs	days	bs
1	Yellow Obernai lime slaked to powder and measured in powder. 1 Sand . . . . . 2 } 3	9	187	15	44	25	22
2	Lime the same . . . . 1 Sand . . . . . 1 Trass . . . . . 1 } 3	6	407	6	407	6	297

Observations on the experiments of Table No. IV.

To make the above experiments, I took the yellow Obernai lime, and slaked it to dry powder by throwing on $\frac{1}{3}$th of its volume of water. I gave it time to cool, and then made two mortars: one, with one part of lime measured in powder and two parts of sand; the other with one part of lime in powder, one part of sand, and one of trass. The mortars remained in

the air during twelve hours, and I then put them in water, where they remained one year, at the end of which time they were broken. The table shows that the mortar of lime and sand, hardened in 9 days and supported 187 lbs, before breaking; it shows also, that the mortar containing lime, sand and trass, hardened in 6 days, and supported 407 lbs. At the end of a month and a half, I repeated the same experiment, using, in powder, the same lime which had served in my first experiment, and which had been lying in an open vessel. I followed, for the second experiment, the same process as for the first: the table shows that in the mortar composed of lime and sand, the hardening was slower, and the resistance much less; and that for the mortar containing trass, the result was the same: again, at the end of two months and a half, I repeated the experiments, using the same lime slaked to powder. The table shows that the hardening was still slower, and that the resistance was still less in the mortar, composed of lime and sand; for by the expression -22, is meant that it could not sustain the weight of the scale-pan, cords, and collar, which was 22 pounds. It also shows that the mortar containing trass experienced a sensible diminution in its resistance, although the time of hardening was the same; but the diminution was not as great as in the mortar made of lime and sand only.

I have thus far gone into particulars, in order that there might be no doubt as to the manner of proceeding: in the following tables, I shall give fewer details. I repeated the above trial, as shown in the following table, using another piece of lime.

Table No. V.

No. of series	Composition of the mortars.		Made immediat'ly		After 15 days.		After 25 days.		After 35 days.		After 45 days.		After 2 months.	
			H	W	H	W	H	W	H	W	H	W	H	W
	Obernai lime slaked to powder and measured in powder.	1	d's	lbs 1	d's	lbs	d's	lbs	d's	lbs	d's	lbs	d's	lbs
	Sand	2 } 3	25	264	11	121	15	77	18	88	18	77	20	88
2	Lime the same	1												
	Sand	1 } 3	12	473	5	330	6	297	10	286	12	304	12	299
	Trass	1												

I made the above experiments, in the same manner as has been explained of Table No. IV; but it will be seen that the periods at which the mortars were made, were brought nearer together. Each experiment in the above table, is divided into two columns: the first contains the number of days, the mortar required to harden, and is marked H; the second column, marked W, expresses the number of pounds that the mortar supported before breaking. I shall adopt, hereafter, this order for all the tables which follow, whenever they contain many columns.

The piece of lime which I used for these experiments, was a petrified cornu ammonis, which was 20 inches in diameter. The table shows that with the first mortar, of lime and sand, the hardening was quite slow, which might be owing to the mortar being made so late as the month of October. It is remarkable that the hardening was not so tardy, for the following, which were made 15 days after slaking. As to the resistance, it was the

greatest with the mortar made immediately. That made after fifteen days, lost about half its strength: beyond this period the mortars offered a resistance varying from 77 to 88 lbs, and the hardening was slow.

If the mortars containing lime, sand and trass be reviewed, it will be remarked that the resistance was greatest, with that made immediately, and that it, afterwards, went on diminishing, but keeping well elevated; and that the hardening of the mortar made at the end of fifteen days, was the most prompt.

When making the experiments of the preceding table, I put aside a piece of the same lime, to allow it to slake spontaneously in the air: after two months I made mortar with it, and obtained the following results: it did not harden till the end of twenty-five days, and the mortar broke with 55 pounds, while the table shows that mortar made with the same lime slaked to powder, and which had been left in that state, during the same time, supported a weight of 88 lbs: this result induced me to recommence the experiment with another piece of lime. I proceeded in this way: I divided the lime into two parts; slaking one of these parts into dry powder, by throwing on a fifth of its volume of water, and leaving the other part to slake in the air. I immediately made two mortars with the lime that had been slaked to powder; in one, I mixed sand only with the lime, and in the other I put sand and trass, and at different periods, I repeated the same experiment. It was not till after fifteen days, that the piece of lime which I had left in the air, gave enough lime in powder to begin two similar sets of mortars. The table, following, shows the results I obtained.

Table No. VI.

No. of series	Composition of the mortars.	Made immediat'ly		After 15 days.		After 1 month.		After 2 months.		After 3 months.		After 4 months.	
		H	W	H	W	H	W	H	W	H	W	H	W
		d's	lbs	d's	lbs	d's	lbs	d's	lbs	d's	lbs	d's	lns
1	Obernai lime slaked to powder and measured in powder 1 / Sand 2 } 3	10	121	6	132	4	132	5	117	5	70	6	66
2	Same lime air-slaked and measured in powder 1 / Sand 2 } 3			+40	77	+40	44	+40	33	+40	-22	+40	-22
3	Same lime slaked to powder and measured in powder 1 / Sand 1 / Trass 1 } 3	2	209	2	(1)	2	389	2	352	2	317	3	195
4	Same lime air-slaked and measured in powder 1 / Sand 1 / Trass 1 } 3					6	352	5	319	6	275	6	455

(1) This mortar broke on being cut; it was very hard, but I could not determine its resistance. Av.

Observations on the Experiments of Table No. VI.

This table shows, first, that the piece of lime used was not of the best quality. I, however, selected, to make the experiments of Table Nos. IV, V and VI pieces of lime of a yellow fawn colour, which appeared to have been submitted to the same degree of calcination. The trials of tables Nos. IV and V, were commenced in the month of October, while those of No. VI, were begun in June; the febleness of the first result of table No. VI cannot, therefore, be attributed to a cold season of the year; nor can it be attributed to weather too warm, because the experiments of table No. II, which gave very good results, were made in June. We are obliged, therefore, to infer, as I have already noticed, that pieces of quick lime of the same quarry, are far from having, all of them, the same quality.

The first mortar of lime and sand, augmented its resistance a little, after fifteen days slaking, and its hardening was much more prompt after a month. Beyond this term, the time of hardening went on augmenting, and the resistance diminishing, in a sensible manner, as in Table No. V. It will be seen that for the mortar made, in the same manner, with air-slaked lime, the hardening was very slow: I pursued the examination for 40 days, and the mortars were not yet hard. The table shows that after 15 days, the mortar gave a resistance much less than that of which the lime had been slaked to powder, and that the resistance went on diminishing. At last it was so weak, that, after three or four months, the mortars were unable to sustain the scale-pan, &c., weighing 22 lbs: the mortars had, in the mean time acquired some consistence, but it was so feeble as to be easily broken between the fingers.

The table shows, also, that mortar composed of lime slaked by water to dry powder, united with sand and trass, hardened promptly. The resistance was less in the mortar made immediately; but it afterwards augmented, and then diminished, and finally, the last gave a great increase. I do not know to what to attribute the anomalies observable in the resistance of these mortars. The last series, made with lime slaked in the air, and in which there is one part of Trass, gave a good result, and the hardening was quite prompt; but the resistance was always less than that of the mortar of which the lime had been slaked by water, to powder. This last mortar also presented, after four months, an anomaly which I cannot explain.

It will be seen by the results of the three tables, Nos. IV, V, and VI, that the presence of trass, corrected the evils resulting from the exposure of the lime to the air, after having been slaked by either process. The air-slaked lime had lost almost all its hydraulic properties; but on mixing it with trass it gave, notwitstanding, very good mortar: this is not surprising, since it appears that exposure to the air, has no other effect, than to cause hydraulic lime to pass to the state of common lime, and this last gives very good mortar when mingled with sand and trass.

The importance of these results, induced me to engage in similar experiments with the Metz lime: they are found as follows.

Table No. VII.

No. of series	Composition of the mortars.	Made immediat'ly.		After 1 month.		After 2 months.		After 4 months.	
		H	W	H	W	H	W	H	W
1	Metz lime slaked to powder and measured in powder . . . . 1 Sand . . . . . 2 } 3	d's 20	lbs 116	d's 30	lbs 44	d's 30	lbs 22	d's	lbs –22
2	Same lime slaked in air and measured in powder . . . . 1 Sand . . . . . 2 } 3			35	88	15	79	9	88
3	Same lime slaked to powder and measured in powder . . . 1 Sand . . . . . 1 Trass . . . . . 1 } 3	3	180*	5	312	11	319	11	385
4	Same lime air-slaked and measured in powder . . . . . 1 Sand . . . . . 1 Trass . . . . . 1 } 3			13	352	9	209*	7	440

Observations on the experiments of Table No. VII.

I made the above experiments like those in the preceding table. If the first mortar, alone, be compared with No. 39 of Table No. III, it will be seen that the lime used in making the above mortars, was much inferior to that of table No III, although the limes appeared to have been burned to the same degree. The trials, in both these tables, were made in the middle of summer, the quantities of sand and all the other circumstances, were the same; nevertheless, the first mortar of table No. VII, shows a resistance of only 116 lbs, while the similar mortar, No. 39 of table No. III, gives 262 lbs.

It is seen that the first series, composed of lime and sand, shows the greatest resistance when the mortar was made immediately, and that the resistance went on diminishing. At the end of four months, this mortar could not support the scale-pan, and it broke easily in the fingers. We see, therefore, that with Metz lime slaked to powder, the result is the same as with Obernai and Oberbronn lime.

The second series was begun, at the end of a month, with the same lime left to slake in the air: the result varied from 79 to 88 lbs.—a resistance inferior to that obtained by making the mortar immediately: but on comparing these results with those of table No. VI, we see that the Metz lime, when air slaked, gave better mortars, than when slaked to powder with water; while it was the contrary with the Obernai lime, of which the two first series of table No. VI were made. It is also remarkable that the hardening was more and more prompt, with the air-slaked Metz lime, while it required more than 40 days for the Obernai lime to harden, in each case.

As to the two series, of table No VII, made with lime, sand and trass, good results were obtained, whether the lime was slaked with water to powder, or was air-slaked; in both cases, the resistances went on increas, ing, and the mortars made of air-slaked lime, gave better results than those

* These mortars were split in the middle.

of lime slaked to powder, which was the contrary of the results of table No. VI.

It is to be regretted, that in the first experiment, of the above table, made with lime, sand and trass, the prism of mortar was found split, and separated into two parts. I put the two pieces together, before breaking; but the resistance must have been diminished by the circumstance, and it amounted only to 180 lbs. The last series, also, gives a mortar sustaining 209 lbs. forming an anomaly, when compared with that which precedes, and with that which follows: this mortar was cracked, but the parts were not separated.

It will appear from all the experiments reported, in tables Nos. IV, V, VI and VII, that the saying of masons, that lime loses its energy in the air, is not, as regards hydraulic lime, without foundation. All those which I have treated above have passed to the state of common lime, after some months, when they had been slaked with water; and the Obernai lime gave the same result when air-slaked. I have sought for the cause that might transform hydraulic lime into common lime.

Chemistry teaches us that lime has not the property of absorbing a new quantity of oxygen; but it is not with lime, properly speaking, that we make mortar; it is with the hydrate of lime; that is to say, with lime slaked by water, which enters into combination with it, forming a new body, which may have new properties. It, therefore, occurred to me that hydrate of lime, or, in other words, slaked lime, might absorb oxygen.

In verifying my conjectures in this respect, I could not employ hydraulic lime, because all those I had at command, contained metallic substances capable of oxidation. I proceeded, therefore, in the following manner: I took a piece of white marble which I assured myself contained almost no iron; I had it calcined in a kiln; as soon as it came from the kiln, I pulverized it, and divided the powder into two equal parts, one of these parts was slaked with a little water, and the other was left in the state of quick lime. I put the slaked lime, and the quick lime, each into one of those cylindrical glass vessels used to measure liquids: I placed each of these vessels in a plate containing a little water. I then covered the two vessels containing the lime, with two other vessels, of the same kind, a little larger—the mouths of these covering vessels being immersed in the water. By these means the lime was no longer in connexion with the atmosphere, and it was connected with the water, only through the column of air that was between the two vessels. After five or six days, the water had mounted perceptibly, between the two glass vessels, which contained the slaked lime, and at the end of twenty days, it had mounted four inches. It must be noticed that at this time, the water had risen very little between the two glasses, containing the quick lime. I repeated this experiment several times, and always with the same result. It, therefore, appeared to me that there had been a great absorption of oxygen by the slaked lime, and a slight absorption by the quick lime. I communicated the results I had obtained to Mr. Coze, professor of Pharmacy at the academy of Strasburg, and requested him to ascertain the quantity of oxygen that had been absorbed in the two cases. To that end, a cylindrical glass, graduated, vessel was taken, and one measure of atmospheric air and two measures of deutoxide of azote (nitrous gas) were introduced. As soon as the air in the cylinder had become red, and the water had risen therein, the residue stood at 112 on the scale of the cylinder. The same thing was done with the air that had been in contact with the quick lime: this gave also a red co-

lour, and the residue stood at 117 of the scale. And then the same trial was made with the air that had been in contact with the slaked lime: the red colour was very feeble, and the residue stood at 132 of the scale. In this last case, therefore, there had been a great quantity of gas absorbed by the lime which had been slaked with a little water. To be convinced that it was oxygen, that had been absorbed, hydrogen gas was mixed with the remaining gas, and the mixture submitted to the electrical spark, without any detonation; from which it resulted, that almost all the oxygen had been absorbed by the hydrate of lime.* That the quick lime absorbed but little oxygen, was doubtless owing to this, that this lime could form a hydrate only very slowly, by absorbing water from the dish through the column of air. It appears to me, then, that it is to the absorption of oxygen we should attribute the singular result, that is obtained by leaving exposed to the air, hydraulic limes that have been slaked with a little water. It has been seen that all the above limes, thus treated, have lost, after a little time, almost all their hydraulic properties, and have passed to the condition of common lime.

It appears to me difficult to explain, why the Metz lime which was slaked in the air gave, at the end of four months, a better result than the lime which had been slaked to powder, while the opposite results were obtained with the Obernai lime. Can this difference be owing to the Metz lime containing the oxide of manganese, which the Obernai lime does not? We know that when oxide of manganese is heated, a great part of its oxygen is disengaged, and that it afterwards absorbs it from the air. When the limestone containing it has been calcined, it is possible that this oxide prevents the lime from absorbing oxygen by its own greater affinity for it. It will be interesting to ascertain if the same thing happens with all hydraulic limes, containing oxide of manganese.

It will be seen by the above experiments how important it is not to leave exposed to the air for any time, hydraulic limes which have been slaked to powder, unless well assured that they are not of the same nature as those which become, in this way, common limes. The same observation applies to hydraulic lime slaked spontaneously in the air: we ought to be certain that the same effect will not result as in the Obernai lime of Table No. VI. Without this precaution we shall be liable to make very bad mortars out of very good materials; and to cause very important and expensive constructions to fail. If good results were obtained in tables Nos. VI. and VII. with trass mortars, of which the lime had been slaked for a long time—whether with water to powder or spontaneously in the air, it was because common lime always gives good mortars with trass, whatever may have been the slaking process, as will be seen in the sequel. The above experiments on the absorption of oxygen by hydrate of lime, are contained in a memoir on hydraulic mortars which I addressed to the Committee on Fortifications in October, 1823; an extract from that memoir was printed in 1824, and inserted in the 7th number of the Memorial de l'Officier du génie.

It remains, before closing this article, to speak of the pebbles of Boulogne, of which, having heard, I caused specimens to be brought to Strasburg: but before reporting the experiments therewith, I must refer to a species of lime, much resembling them, well known in England.

In 1796 Messrs. Parker and Wyatts obtained a patent in London for

*It appears from this, that there is reason for forbidding the occupying of newly plastered houses—as they must be unhealthy. Au.

making a particular kind of lime, which was first called *aquatic cement*, and afterwards, improperly, *Roman cement*. At this moment, this cement is an article of considerable commerce, and is sent even to the Indies. This substance has the property of solidifying, like plaster of Paris, almost instantly, when used in the air, and of taking, very soon, a strong consistence in water. It is mixed with 2, 3, 4, and 5 parts of sand, to 3 parts of cement, according to the use to be made of it.

Captain Le Sage, of the Engineers, has brought to notice a substance like the English stone: it was found by an Englishman, by accident, about 20 years ago, upon the shore of Boulogne: a description is given of it in the 2nd vol. of the *mémorial de l'Officier du génie*. This substance is thrown upon the shore by the sea, in the form of pebbles, seldom weighing more than $2\frac{1}{2}$ lbs. Their colour is, in general, a yellowish and dirty gray: several of these pebbles have the surface, to the depth of $\frac{8}{10}$ of an inch, of a rust colour: these stones are of a very fine grain, and they are hard. They are found only in small quantities, and here and there, so that they are used at Boulogne only, where they are advantageously employed in hydraulic works. Mr. Drapier made the analysis of these, and Mr. Berthier of the English stone: from a comparison of these two stones, presented by Mr. Berthier in the same table, after deducting the carbonic acid, and the water which is evaporated by calcination, there remains of the substances employed as lime, viz:

The English stone—lime, 0.554; clay, 0.360; oxide of iron, 0.086.
The Boulogne pebbles— do. 0.540; do. 0.310; do. 0.150.

I ought to add that the analysis gave for the English stone some thousandths of carbonate of magnesia, and of manganese, which are not found in the Boulogne stones: but these quantities being very small, we see that the two are essentially the same. I now give a table of the experiments made with the Boulogne pebbles.

Table VIII.

No. of the mortar.	Composition of the mortars.	No. of days required to harden in water.	Weight supported before breaking.
		days.	pounds.
1	Boulogne pebbles alone, ordinary burning,	$\frac{1}{2}$	108
2	do.	$1\frac{1}{2}$	132
3	do.	$1\frac{1}{2}$	156
4	do. 2 / Sand 1 } 3	20	37
5	Pebbles as above 2 / Sand 1 } 3	22	33
6	Pebbles as above 1 / Sand 1 } 2		22
7	Pebbles as above 1 / Sand 2 } 3		— 22
8	Pebbles as above 1 / Sand 1 / Trass 1 } 3		22
9	Pebbles as above 1 / Trass 2 } 3		26
10	Pebbles alone, air-slaked for a month	15	48
11	Pebbles alone, slaked a month with one-fifth the volume of water.		— 22
12	Pebbles alone, highly calcined	12	286

Observations on the Experiments of Table No. VIII.

The lime of the first eleven numbers of Table No. VIII. was calcined in a lime kiln, being placed with lime-stone producing fat lime: it was, therefore, calcined with the degree of heat required for burning fat lime. The first three numbers are of the lime alone. We were obliged to pulverize the pebbles as they came from the kiln, because, on throwing on water, instead of being reduced to powder, like fat limes, and ordinary hydraulic limes, they absorb and solidify it. These pebbles afford no vapours on slaking, and give out very little heat. This lime requires less water, than ordinary hydraulic lime, to be reduced to paste. In the above experiments, the pebble lime was reduced to paste a little stiffer than for ordinary hydraulic mortar, because it was perceived that this lime required less water. The first three numbers were made in the same manner; differing only in this, that the first number was put in water after having been formed into paste and exposed three hours in the air; the second number, after two hours exposure, and the third, immediately: it will be seen that this last gave the greatest resistance, but that which gave the least resistance was the quickest to harden, which arose from its having already acquired in the air an evident consistence. We see then, that in constructions in water, when this lime is to be used, it is important to prepare no more mortar than can be used immediately. The numbers from 4 to 9, inclusive, are mixtures of pebble lime with sand in several proportions, and the numbers following contain trass. The hardening was so slow that I was unable to observe them all. We see that the resistance of all these mortars was feeble. It is singular that trass, which gave such good results with all the hydraulic limes I have tried, gave such feeble results with this pebble-lime: it appears that this lime does not bear, with advantage, any mixture. The pebbles of No. 10 were broken up and left to slake in the air for a month; those of No. 11 were exposed during the same time to the air, after having $\frac{1}{8}$th of their bulk of water thrown on them. We see that the air-slaked lime gave a very weak result, and that which was slaked with water gave a still feebler, since it was unable to sustain the weight (22 lbs.) of the scale pan, &c.

The weak resistances, shown in the first numbers of the above table, gave me the idea of another experiment, after a higher calcination: to this end, I exposed a piece, in a lime kiln, opposite one of the flues intended to convey the heat through the kiln: the pebble was highly burned; it took a deep rust colour, while those less calcined were of a yellow fawn. No. 12, which is the experiment made with the highly burned pebble, was treated like No. 2, being left, in the state of paste, in the air for one hour before being immersed. We see that the resistance was very good, since this No. 12 supported the weight of 286 lbs. before breaking; but the hardening was quite slow. This trial was made with the remainder of my stock of pebbles; otherwise I should have made several essays to ascertain whether by mixing this strongly calcined lime, with sand and trass, I should obtain better results than those given when less burned.

Taken all together, the experiments of table No. VIII. show, that the lime derived from Boulogne pebbles is a hydraulic lime presenting in several respects, great differences from the other hydraulic limes which I have treated. The ordinary hydraulic limes of which I have spoken, demand only the same degree of heat as is required to calcine fat lime, and it is perhaps proper to calcine them a little less. When these limes are but little burned, they form a good body with sand, and give very good results with

trass: if the same degree of heat be given as was received by the pebbles in experiment No. 12, they sustain an incipient vitrification, and give only weak results. A circumstance, quite remarkable, is, that when I treated these limes without mixing them with any other substance, those numbers which hardened the quickest, gave the least resistances, while those which took the longest time to harden, gave results much superior to the others. All these experiments were, notwithstanding, made at the same season—the beginning of the winter. The differences which I have pointed out, between Boulogne pebbles and ordinary hydraulic lime, induce me to suspect there might be, in the pebbles, some substance that escaped detection in the analysis. I have, moreover, attempted to recompose this lime, by a mixture of the substances given by the analysis: on burning it, I had ordinary hydraulic lime, and none of the peculiar results given by the pebbles. To ascertain if the pebbles contained any substance not mentioned in the analysis, I took one of the crude stones, broke it, and examining it with the magnifier, I noticed some very small crystals which did not appear to be calcareous. I pulverized these particles and put them, for some time, in distilled water: I then filtered and concentrated the liquid: it had a decided salt taste, and on leaving it at rest, I obtained quite a quantity of crystals of muriate of soda, which had been previously noticed as being present. It appeared to me that this muriate of soda, contained some crystals of carbonate of soda. This did not surprise me, since we know, from the fine observations of Mr. Bertholet, that Natron, which is brought from Egypt, is formed by the decomposition of muriate of soda, by the carbonate of lime which exists in the bottom of the lakes, situated in the desert of Thaïat to the west of the Delta. In winter, these lakes fill with salt water which enters through the bottom and rises about seven feet; on the return of warm weather, the water of the lakes is completely evaporated, leaving on the bottom, which is calcareous, a stratum of natron, which is a mixture of sea salt, (muriate of soda) sulphate, and carbonate, of soda, constituting the soda of commerce: this natron is broken up from the bottom with iron bars. The muriate of soda which exists in the Boulogne pebbles in notable quantity, and the crystals of carbonate of soda which I thought I discovered, induced me to make several experiments (which I shall soon report,) by adding a little soda and potash to a mixture of lime and clay, which I then burned in order to obtain an artificial hydraulic lime.

Mr. Pitot du Helles, formerly an officer of Engineers, now living at Morlaix, sent me, lately, a singular substance which furnishes hydraulic lime, and seems to me to bear analogy to the Boulogne pebbles. This substance appears to come from the fragments of a species of madrepore of which the geological position does not seem to be well known, but which appears to exist outside the roadstead of Morlaix: these fragments are brought by the currents, and form considerable deposits in the roadstead, whence they are raised with the drag, by the boatmen: the farmers make considerable use of it as a manure for certain soils, and give it the name of *merle*. It is in very small and contorted fragments; its colour is a dirty white; it is light. Mr. Grimm, *pharmacieur en chef de la Marine*, at Brest, made an analysis of this substance, which contains: carbonate of lime, 5.25; sand, 1.35; water and organic matter, 3.30; oxide of iron and phosphate of lime, traces; loss, 0.10. Mr. Pitot made lime of this substance, and I made some trials with the specimen he sent me.

When water was thrown on the calcined *merle*, it was almost a quarter of an hour before any heat was manifested, and that so slight that a little

vapour was barely perceptible. This lime does not reduce well to powder by slaking; it would be necessary to reduce it by pestles: in this, it presents similar character to the Boulogne pebbles.

I reduced a portion of this lime to paste with water, and placed it at the bottom of a glass filled with water: the hardening was complete at the end of six days, although the experiment was made towards the close of November. I mixed a portion of this lime with an equal quantity of sand: the hardening took place in eight days; and it seemed to me that this lime would not bear much sand. I was not able to ascertain the tenacity of mortar made of this lime; but the manner in which it hardened, and the degree of induration it had acquired in a short time, led me to think that it is a very good hydraulic lime; and that it will be of great advantage in all hydraulic constructions on the coast of Bretagne. I am astonished that alumine was not found in this lime, so eminently hydraulic.

(TO BE CONTINUED.)

Physical Science.

Electro-Magnetic Telegraph.

On a recent visit to New York, the Editor had an opportunity of examining the apparatus constructed by Professor Morse, of the University of that city. The subjoined account of it from Prof. Silliman's Journal for the present month (October) will afford a correct idea of the manner in which the communications made by it are recorded, and read off. Our examination of it was subsequent to the addition of the thousand feet of wire, and, although in an experimental essay of this kind we do not look for a perfect arrangement of the instrument employed, yet its operation might be pronounced perfect, and no reason was perceived why its indications may not be given at greater distances than by the ordinary telegraph, whilst they will be uninfluenced by the causes which limit the application of the latter to day-light, and clear weather.

1. *Morse's Electro-Magnetic Telegraph.*—While a contest is waging in several contries of Europe—in England, Scotland, France and Germany, for the discovery and invention of the Electric Telegraph, it may not be amiss to state, that America also claims to be an independent discoverer and claimant for priority in the invention. The dates, the names of the inventors, and other circumstances will doutless ere long be published, and then the world can judge between the conflicting parties. In the mean time it is well ascertained, that Prof. Morse, of the New York City University, conceived and planned, five years ago, an electric Telegraph, while on his passage home from France, and immediately on his landing, he commenced the machinery. Early last spring, *in April*, the general features of his plan were very extensively published in the newspapers, and very lately, *in August*, we learn that several telegraphs on the basis of electricity are in various stages of progress in Europe.

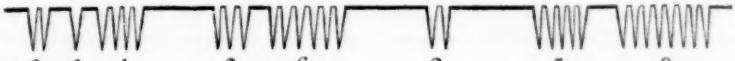
The distinguishing features of Prof. Morse's telegraph are a *Register*, which permanently records in characters easily legible the fullest communication, and the use of but *one wire* as a conductor; although for greater convenience of communicating at all times, and of having a whole circuit at command from each extremity of the line he will use four wires.

On Sept. 2d, Prof. Morse tried an experiment with a circuit of copper

wire one thousand seven hundred feet in length, and of the minimum size of No. 18 wire. The record of the Register was sufficiently perfect to demonstrate the practicability of the plan. On the 4th of September some slight changes were made in the machinery, when the Register recorded perfectly the following signs:

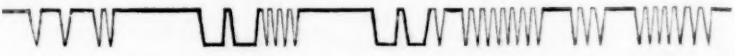
Specimen of Telegraphic writing made by means of electricity at the distance of one-third of a mile.

Successful experiment with Telegraph.
2 1 4 3 6 2 5 8



2 1 4 3 6 2 5 8
Successful experiment with Telegraph.

September 4th, 1837.
112 0 4 0 1837



1 1 2 0 4 0 1 8 3 7
September 4th, 1837.

The *words* in the diagram were the intelligence transmitted.

The *numbers*, (in this instance arbitrary,) are the numbers of the words in a Telegraphic dictionary.

The *points* are the markings of the Register, each point being marked every time the electric fluid passes.

The Register marks but one kind of mark, to wit, (V). This can be varied two ways. By intervals thus (V VV VVV) signifying one, two, three, &c., and by reversing thus (Λ); examples of both these varieties are seen in the diagram.

The single numbers are separated by *short*, and the whole numbers by *long intervals*.

To illustrate by the diagram, the word "successful" is first found in the dictionary, and its telegraphic number 214 is set up in a species of type prepared for the purpose, and so of the other words. The types then operate upon the machinery and serve to regulate the times and intervals of the passages of electricity. Each passage of the fluid causes a pencil at the extremity of the wire to mark the points as in the diagram.

To read the marks, count the points at the bottom of each line. It will be perceived that two points come first, separated by a *short* interval from the next point. Set 2 beneath it. Then comes one point likewise separated by a *short* interval. Set 1 beneath it. Then come four points. Set 4 beneath it. But the interval in this case is a *long* interval, consequently the three numbers comprise the whole number 214.

So proceed with the rest until the numbers are all set down. Then by referring to the Telegraphic Dictionary, the words corresponding to the numbers are found, and the communication read. Thus it will be seen that by means of the changes upon *ten* characters, all words can be transmitted. But there are *two points* reversed in the lower line. These are the *eleventh* character, placed before a number to signify that it is to be read as a *number*, and not as the representative of a word.

Since the 4th of September, one thousand feet more of wire No. 23 have been added, making in all two thousand seven hundred feet—more than

half a mile of a reduced size of wire; the Register still recorded accurately.

Arrangements have been made for establishing a circuit of several miles, and for constructing new and accurate machinery. Prof. Gale, of the New York City University is engaged with Prof. Morse in making some interesting experiments connected with this invention, and to test the effect of length of wire on the magnetizing influence of voltaic electricity.

To the foregoing notice, we append an article published in Thomson's Annals of Philosophy, Vol. VII, p. 162, first series, February 1816. This article is from the pen of Dr. John Redman Coxe, of Philadelphia; and it is believed that the idea of the employment of Galvanism, as a Telegraph, which it suggests, was then original. Those who are acquainted with the history of the progress of Electricity, as evolved by the ordinary machine, are aware that experiments had been made with a view to its employment for a similar purpose; but from the inherent difficulties of the subject, the project had been abandoned.

It is not pretended that the state of our knowledge, on the subject of Galvanism, was such at the time the foregoing suggestion was made, as would have enabled any person to apply it practically, this, if done, will be due to the recent discoveries on the subject of Electro Magnetism, a subject which has been very successfully pursued by the Philosophers of our own country, and particularly by Professor Henry, of Princeton. As some of the Philosophers of Europe, are disputing upon the question of the authorship of the proposition for the employment of Galvanic Electricity, Telegraphically, we have thought that it would not be altogether inopportune, or uninteresting, to republish the article above referred to.

"*Use of Galvanism as a Telegraph; in an extract of a letter from Dr. J. Redman Coxe, Professor of Chemistry, Philadelphia.*

"I observe in one of the volumes of your annals of philosophy, a proposition to employ Galvanism, as a Solvent for the urinary calculus, but which has been very properly, I think, opposed by Mr. Armiger. I merely notice this, as it gives me the opportunity of saying, that a similar idea was maintained in a thesis, *three years* ago, by a graduate of the University of Pennsylvania.

"I have, however, contemplated this important agent, as a probable means of establishing Telegraphic communications, with as much rapidity, and perhaps less expense, than any hitherto employed. I do not know how far experiment has determined Galvanic action, to be communicated by means of wires; but there is no reason to suppose it confined, as to limits, certainly not as to time. Now by means of apparatus, fixed at certain distances, as telegraphic stations; by tubes for the decomposition of water and of metallic salts, &c. regularly ranged, such a key might be adopted as would be requisite to communicate words, sentences, or figures, from one station to another, and so on to the end of the line. I will take another opportunity, to enlarge upon this, as I think it might serve many useful purposes; but, like all others, it requires time to mature. As it takes up little room, and may be fixed in private, it might in many cases, of besieged towns, &c. convey useful intelligence, with scarcely a chance of detection, by the enemy. However fanciful in speculation, I have no doubt that sooner or later, it will be rendered useful in practice.

"I have thus, my dear sir, ventured to encroach upon your time, with

some crude ideas, that may serve to elicit some useful experiments in the hands of others. When we consider what wonderful results have arisen from the first trifling experiments, of the junction of a small piece of silver and zinc in so short a period, what may not be expected from the further extension of galvanic electricity! I have no doubt of its being the chiefest agent in the hands of nature, of the mighty changes that occur around us. If the metals are compound bodies, which I doubt not, will not this active principle combine those constituents in numerous places, so as to explain their metallic formation? and if such constituents are in themselves aeriform, may not galvanism reasonably tend to explain the existence of metals in situations to which their specific gravities, certainly do not entitle us to look for them?

Bibliographical Notice.

Elementary principles of carpentry, by Thomas Tredgold, Civil Engineer; first American, from the second London Edition, corrected and considerably enlarged. E. L. Carey and A. Hart, Philadelphia, 1837.

This work forms an excellent elementary Manual, for the carpenter, as well as a valuable book of reference, for the Architect, or the Engineer.

The principles laid down by Mr. Tredgold, relating to that branch of the art, which is particularly directed to the support of weight or pressure, are all founded upon the immutable laws of mechanics, and explained with great perspicuity:—The strength of various kinds of timber to resist *tension, compression,* or *cross strains,* is fully investigated, and copious tables are given, exhibiting the results of experiments, on the strength and cohesion of almost every description of wood.

One of the most important divisions of the work, is that which is devoted to the investigation of the *nature and properties of timber.* Many very curious and interesting facts, connected with the growth of trees, are mentioned, and much valuable information is furnished on the subject of felling and seasoning timber, the prevention of decay and dry rot, and the durability of wood in different situations.

The tables of scantlings for roofs, trusses and girders of various span, are also of great utility to practical men. These tables have, however, been prepared for the yellow fir of Europe; it will, therefore, be necessary, in adapting them to other woods, to have reference to the strength of the material to be used, in comparison with the strength of yellow fir; these calculations are rendered easy by the tables giving the *properties* and the *specific gravities* of timber; which tables (by the way) are by no means the least useful portion of the work.

A table of the specific gravities of other substances, forms also a part of this valuable production; and the whole work is illustrated by engravings, executed in a very superior style.

Mechanics' Register.

THE PATENT OFFICE.

The notices of Patents, granted by the United States, have been omitted since the destruction of the Patent Office, with all its Records and Models, on the 15th of December last; these notices we now resume, and on doing

this, the Editor offers the following remarks, and explanations, upon certain points, which appear to him to be of interest to those in any way concerned in patents for useful inventions.

It has long been the practice of the Editor, to delay a particular notice of patented inventions, for several months after the time of their being granted; a course necessary from various considerations, but more particularly for the two following: First. Patents are not granted in foreign countries for inventions, descriptions of which have been previously published; the arrival of this Journal in England, or in France, one day before the sealing of a patent, would destroy the grant, and cause the loss of the large amount of fees paid into the offices. In applying for such patents, there is usually much delay from the want of proper information; and a considerable length of time must necessarily elapse, in the foreign offices, between the times of application for, and that of the sealing a patent. This alone, would be a sufficient reason for the delay; but there is a second of some personal importance, to the Editor himself, as well as to patentees. It is the practice of the Editor, to give his own opinions, freely and fully, respecting the value of inventions, and it is his wish, not to interfere with the interests of the inventor, by forestaling public opinion, before he shall have had time to test the value of his supposed improvements, by a practical test.

Under the operation of the acts of July 4th, 1836, and of the 3rd of March, 1837, by which all former laws relating to patents, were repealed, all applications for patents are subjected to a critical examination in the Patent Office, and if the things claimed, are known to be old, or if that which is proposed to be done, contravenes the established laws of Mechanical and Chemical Philosophy, the patent is refused; subject, however, to an appeal from the decision of the office. Under this law, there are two examiners appointed, of whom the Editor is one, and it will, therefore, be manifest that the tone of his animadvertions, must be modified by the existence of these circumstances. Under the official examination, a large class of applications, will be rejected, and never, therefore, meet the public eye; this will be a source of sufficient mortification to those who have dreamed of reaping a golden harvest, without the lash of criticism. It must not be inferred from these remarks, that our list of patents will become a mere detail of approved inventions; the fact will be far otherwise; for although the office is now possessed of judicial power to a certain extent, and can arrest what is old, what is contrary to the laws of nature, and what is deemed altogether trifling, it would be stepping out of the line of its duty, and transcending its legitimate powers, to refuse a patent for an alleged improvement or invention, because the Examiners, or the Commissioner, may believe the thing worthless. They are not to act under the guidance of mere opinion, but must, and ought to, give the sanction of the office in all cases where there is apparent novelty. The utility is a question to be subsequently settled between inventors and the public. Nor is the absolute amount of novelty, a question for this office, as a very large amount of utility, not unfrequently results from a trifling change in the construction of a machine; the duty of the office in this case, is to see that the claim does not embrace more than the invention of the applicant; and, whenever there is a doubt respecting the propriety of rejection, the decision of the office will always be given in favour of the claimant, who, in such case, has an undoubted right to a final determination of his interests, by a Court and Jury.

The number of patents hereafter issued for quack medicines, will, under the operation of the existing law, be much diminished, and, perhaps, it would be for the public benefit, could they be entirely cut off from the sanction of an exclusive right; but this cannot be done in all cases. Where compounds substantially the same with those known to the practicing Physician, are attempted to be patented, they will, of course, be excluded, but they must be granted when the compound appears to possess novelty, and is not manifestly of such a nature as to produce public injury: the Journal, however, will fully make known every recipe for a patent medicine, accompanied by such remarks as may appear just and proper.

The enquiry is frequently made, what proportion of the applications under the present law, are successful? Without taking the trouble to estimate the precise number, it may be stated, that not one third pass as originally presented, a large number being returned for amendment, and modification of the claim; probably about one half of those applied for, are eventually granted.

The next number of the Journal, will contain some information respecting the restoration of the records and models of the office, as it appears that the circular issued upon this subject, has not, generally, been clearly understood.

American Patents.

LIST OF PATENTS WHICH ISSUED IN JANUARY AND FEBRUARY 1837.

With remarks and Exemplifications by the Editor.

1. For improvements in *Reaction Water Wheels*, Gideon Hotchkiss, Windsor, Broome County, New York, January 19.

Patents for reaction wheels, are so numerous, that there does not now appear to be any thing left to claim beyond certain minor peculiarities in individual parts, which, although they may present sufficient novelty to render it proper for the office to grant a patent therefor, are not likely so far to change the character of such wheels, as to justify their adoption where economy of water is an object of any importance, although they may thereby be rendered less objectionable, in those situations where circumstances may render their employment convenient.

According to the improvements, now proposed, there are to be several wheels placed upon one horizontal shaft, which wheels are, in their general construction, exactly like those used by Calvin Wing, and by several others whose plans have been described in this Journal. In the drawing before us, there are four such wheels combined together; and the first claim made, is to a peculiarity, in the form of the opening in the head through which the water is admitted. Instead of being a circular opening, extending to the inner edges of the buckets, it is to be scolloped, there being as many hollow curves as there are buckets, the edges of which curves are to be sharpened, that they may "cut away opposing substances, such as leaves, straw, wood, &c." We apprehend that this claim must stand upon the novelty of the structure, without any aid from its utility. The second claim is to the so casting, or forming, the eye of the wheel, that it may be reversed on the shaft, which will reverse the direction of its motion. This is a small affair, the value of which we leave to the estimation of those concerned. The third claim is to "the manner of constructing the wheel with cast-iron hub and arms." This is the child of the second claim, and

consists in the particular mode of forming the parts for the purpose of reversing, as therein noted. The fourth claim is to "the use and application of the combined pivot gates, or valves, for letting on or shutting off the water." This is a claim to the particular arrangement of the levers, for opening or closing the valves or gates, and secures nothing but this peculiar mode of doing a thing, which may be effected in other ways. The sixth claim is to "the making of the wheel case portable, or entirely independent of the mill frame, with the axles of the wheels supported by, and turning in the same." This latter device, would seem to render the second, third, and fourth unnecessary, as by turning the portable case, end for end, the reversing operation would be completely effected.

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2. For *Fastenings for Harness;* Warner Hayden, New Milford, Susquehanna County, Pennsylvania, January 21.

The patentee says, that he effects the object proposed, of fastening harness, "by means of metallic clasps, screws, and pins, which I use as substitutes for the buckles, usually employed for that purpose." The improvement is not confined to any particular part of the harness, but the clasps and pins are to be used, both on the traces, and on the smaller straps. The clasps may be made more ornamental than buckles, and are simple in construction, consisting of a flat plate with a staple, or staples upon it, through which the end of a trace, or strap, is to be passed, and in which it is confined by a sufficiently stout screw pin, or pins, passing through the staple and leather strap, and screwing into the plate below. One screw is generally deemed sufficient; but for traces, or other parts where there is much strain, two may be employed.

In some cases it is proposed to have two staples upon a plate, to receive the strap, and between the two staples, to have a pin firmly attached to, and rising from the plate, so that by bending the strap, the pin may enter one of the holes, when, by straightening the strap, the fastening will be completed. The claim is to "the within described manner of making and using such fastenings for harness, wherever buckles have been heretofore employed for that purpose."

We have examined specimens of harness thus fastened, and the plan is certainly recommended both by its beauty and neatness; but we have heard some objections made to it, by practical men, and have not learnt, whether these have been found valid upon actual trial. It has been said that the bend of the leather, over the rim of the buckle, is essential to the sustaining the strain upon the tongue, or pin, without which the leather, or pin, would be likely to give way. It has been apprehended, also, that the screws would be much more liable to come loose, than a buckle, a circumstance calculated to produce very injurious effects.

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3. For a "*Saddle for removing the sick and wounded,* and answering also as a pack saddle; Hezekiah L. Thirtle, City of New Orleans, January 21.

A long saddle tree, is to be adapted to the back of a horse, and from each end of this, two springs are to rise, like coach springs, which are to sustain the four corners of an iron cot frame, sufficiently long and wide for a man to lie on, when it has a suitable bottom strained on it; sides and ends of leather are to be raised around the cot frame. Straps are to be attached to the apparatus, to steady and secure the person to be carried. The claim is

to "the before described saddle, for conveying the sick and wounded, and answering also as a pack saddle, when not used for the above purpose."

We are told that this contrivance is considered new, by such officers of the army who have examined it, and that it is believed it may be very advantageously employed in the present war in Florida, where, from the nature and state of the country, there are but few roads which can be travelled by wheeled carriages.

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4. For *Transparent Door Plates*; William C. Austin, Greenville, Augusta County, Virginia, January 31.

Plate glass is to be set in a suitable metallic frame, one surface of which glass is to be ground, to render it semi-transparent. Upon this, plate letters of metal are to be affixed, by pins passing through holes drilled in the glass, or in any other suitable way. A hole is to be cut through the door, upon which the plate is to be placed, so that by means of an ordinary entry lamp, the name may be rendered visible at night, whilst the glass will form an agreeable ground for the letters by day.

The Patentee says, "I do not claim the use of door plates formed of transparent plates of glass, lettered on the back, and contained within metallic frames, as of my invention; but what I do claim, is an improvement on such plates, by cutting a hole through the door, and affixing them in such manner as that a light within the passage, or hall, shall, at night, shine through them for the purpose herein set forth."

Names and other devices, have been painted upon glass, both transparent and ground, but probably there may be novelty enough in the proposed plan of affixing ground glass, as door plates, to sustain a patent; and to Physicians there may be utility in their employment, but their extensive adoption is a very doubtful affair.

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5. For improvements in *Cooking Stoves;* Benjamin W. Wood, Bath, Steuben County, New York, January 31.

The grate upon which the fuel rests, in this stove, is made capable of being raised and lowered by means of racks and pinions; there being a rack at each end of the grate, and two pinions on a shaft crossing the front of the stove, and taking into the teeth of the racks. Variations in the manner of effecting this, are pointed out. At the back of the fire-place, there is to be an oven, having a circular plate, forming the principal part of its bottom, which plate is capable of revolving, there being teeth on its under side into which a pinion is geered, for the purpose of turning it. The upper plate of the oven is also circular, and revolves with the lower, whilst it is capable, also, of being raised or lowered, by means pointed out by the patentee.

Instead of such grooves, or edges, as are ordinarily cast on the edges of the plates, to hold them in place, there are dovetail grooves cast on the upper and lower plates, into which the side plates may slide, and become securely fixed without other fastenings; the end plates are similarly fastened.

To cause the back plate of the fire chamber, to become highly heated, so that it may communicate sufficient heat to the oven, there are ribs formed on its surface, to prevent the direct contact of the wood, and allow a free course to the draught and flame.

Claim.—"I do not claim to be the inventor of the raising of a grate, by means of a windlass or other analogous contrivance, this, standing alone, not being

new, nor do I claim the mere making of a circular plate, to revolve upon the bottom of an oven, by means of a rack and pinion, this also having been effected, although by other means; but what I do claim, is the combination of the apparatus for raising the furnace grate, with a stove, having a revolving bottom plate to the oven thereof, with the second plate herein described which is capable of being raised and lowered, and of revolving with the bottom plate, by means analogous to those described, and for the purposes herein set forth. I claim also, the manner of putting together of cast iron stoves, of any description, by means of dovetailed tongues and grooves; and the placing of ribs, or ridges, up the back plate of the furnace to admit the flame and heated air, to come into contact therewith."

Whatever apparent utility there may be in the contrivances for adjusting the fire, and the oven, in the above described stove, will be more than counterbalanced by its real complexity. In articles of perpetual use, and under the supervision of such cooks as are said to be the envoys of his satanic majesty, the utmost simplicity of construction is requisite, or derangement of parts, and an additional excuse for spoiling the meat, will be the sure result. The best part of the foregoing contrivance, appears to be the putting of stoves together by dovetailed grooves, instead of by rods and screw-nuts.

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6. For a *Machine for Boreing and Mortising*, John H. Power, Norwalk, Huron County, New York, January 31.

This machine, is principally intended for boreing and mortising holes in posts, for post and rail fence. It consists mainly of three screw augers, placed side by side, in a frame, with pinions on their shanks, so geared that by turning a crank, the three augers will revolve, the middle one in a direction the reverse of the other two. To cut the three holes into one, there are four chisels, which advance with the frames, and are so situated that they pare off the redundant stuff between the holes. The middle, or left hand auger, has two projections on it, which are claimed, but the object for which they are formed, is not very apparent. The combination of the augers and chisels, is also claimed.

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7. For an improvement in the construction of *Elliptical, or double Bow Springs, for Carriages;* William Croasdale, Hartsville, Bucks County, Pennsylvania, February 3. (See specification.)

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8. For a *Washing Machine*; William Hovey, Worcester, Worcester County, Massachusetts, February 4.

This machine in its general construction, does not differ materially from some of the host of washing machines, previously patented. A roller ten or twelve inches in diameter is to revolve in the trough, which is to contain soapsuds, and the clothes to be washed. This main roller is fluted from end to end, and is made to revolve in the trough, by means of a winch. Beneath this main roller, there is to be a hollow segment surrounding about five-twelfths of its surface, and within this segment, there are to be rollers of about an inch in diameter, which turn upon gudgeons, and are so placed as to correspond with the flutes on the main roller. This segment is to be borne up against the main roller by spiral springs, and to enable it to adapt itself to the main roller, it is divided into two parts, meeting each other along the bottom. A very elaborate description is given of the particular construction of the divided segment, its slides and springs, as these, but

principally the divided segment, are the subjects of the claim. The clothes are to be washed, by passing and repassing between the main roller and the segment of rollers.

There is not any thing in this machine, to insure it against the fate which has awaited the whole tribe of washing machines, that of sinking into an early grave, unwept, excepting by their immediate parents, whose regrets will not unfrequently be accompanied by a wish, that the child had never been born.

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9. For an improvement in a *Machine for Weighing Heavy Bodies;* Erastus and Thaddeus Fairbanks, St. Johnsbury, Caledonia County, Vermont, February 10.

A patent under a similar title, was obtained by the Messrs. Fairbanks, on the 13th of June, 1831, which patent was surrendered and reissued under an amended specification, on the 6th of March, 1834; this last has also been surrendered, and that before us issued in its stead. Other patents were also obtained for improvements on their weighing apparatus, by the same gentlemen, one of which was dated in February, and another in September, 1832. These also have been surrendered, and in lieu of them, seven new patents have been obtained; the main object of the Patentees, in adopting this course, it is believed, has been to separate their several improvements from each other, conceiving that in case of infringement of their rights by the adoption of certain parts of them, there would be an advantage, in prosecuting a suit, upon a patent unincumbered by improvements not in question. The former specifications have been noticed, and one of them inserted in full, in this Journal, soon after they were granted, we shall, therefore, give but little more, than the claims under the present specifications. The claim in the first of the series, is to the knife-edge hinges, constructed in the manner herein set forth, and the pins denominated rockers, as shown in the drawing."

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10. The title and date of this patent, are the same with the foregoing, and the subject matter of it was contained in the original patents, surrendered as above. The following is the claim.

"We do not claim the invention of knife edge bearings, but we do claim the employment of two levers, connected together at a point equidistant from their axes, or at the point of their relative power, in combination with two pairs of knife edge hinges, or bearings, whose knife edges are parallel lines."

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11. For improvements in the mode of constructing, and *Weighing by, the Steelyard Balance;* re-issued on the surrender of the Patent of September 22, 1832, February 10.

Claim.—"We do not claim the movable weight attached to the beam, for the purpose *simply* of obtaining an exact counterpoise to the platform, as heretofore used; but we do claim it in the positions above described, for the double purpose of obtaining an exact counterpoise to the platform, *and* of putting the beam into equilibrium."

The original specification of this patent will be found at p. 399, vol. XI., of this Journal, with a cut of the apparatus. We are apprehensive that the claim now made will not be found to obviate the objections to which the former may have been liable, or, at all events, that the form given to it is

itself vulnerable. Patents are not granted for results, but for the means by which the desired results are attained; the decisions of the courts are clear upon this point. The foregoing claim is manifestly to the result obtained by the manner of attaching the movable weight; and the terms employed would seem to indicate that such weights, so attached, had been formerly employed for the purpose of balancing the platform only, but that the patentees had discovered that the same weight served also to put the beam into equilibrium, and claim a patent for this discovery. This, however, is not the kind of discovery for which patents are granted. We have always believed, and still believe, that the particular manner of attaching the movable weight, described in the specification, was the invention of the patentees, and if so, the results, whatever they may be, belong to them, because they have a right to the means by which they are obtained.

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12. For improvements in the mode of constructing, and *Weighing by the Steelyard Balance;* re-issued on the surrender of the Patent of September 22, 1832, February 10.

Claim—"What we claim, and that for which we ask a patent, is the combination of the use of the graduated weights as above described, to be suspended at the end of the beam, indicating the hundreds and thousands of pounds, with a movable poise traversing the arm of the beam, and showing the intermediate pounds."

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13. Improvement in the *Machine for Weighing Heavy Bodies;* Patent of February 21st, 1832, surrendered, and the present re-issued February 10.

Claim—"What we claim as our invention, and as our improvement upon the machine formerly patented by us, is the variation in the manner of arranging the levers as described. The arrangement being that of having the longer lever, with its rocker, triangular, extending through the centre of the machine; and the shorter lever, while it forms but one power, is constructed with two arms, so fitted as to be suspended to the longer lever at a point equidistant from the axis of each, thus preserving the simple combination of two levers, as claimed in our former patent."

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14. Improvement in the *Machine for Weighing Heavy Bodies;* Patent of February 21st, 1832, surrendered, and the present re-issued, February 10.

Claim.—"What we claim as an improvement upon the weighing machine formerly patented by us, is the mode of obtaining a vibrating motion in the supporting power by means of the standards (E E) which are used instead of the fulcrum used in our first machine."

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15. Improvement in the *Machine for Weighing Heavy Bodies;* Patent of February 21st, 1832, surrendered, and the present re-issued, February 10.

Claim.—"What we claim as new, and as an improvement on the machine formerly patented by us, is the variation of the manner of connecting the two levers in the centre, (whether they be composed of one or more parts each) to wit, by means of knife edges resting in pendulous rings, or circles."

16. For a composition for *rendering Boots and Shoes Water-proof;* Patrick G. Nagle, City of Philadelphia, Feb. 10. (See Specification.)

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17. For improvements in the *Knitting Machine;* for which Letters Patent were granted on the 5th day of March, 1831, John McMullen, Sinking Valley, and Joseph Hollen, Jr., Huntingdon county, Pennsylvania, February 11.

It will be readily believed that a machine for knitting stockings must be one of cosiderable complexity, and that a correct idea of its individual parts cannot be given in words only. Even with the drawings it would be quite a study to trace the action of that before us. We shall not, therefore, attempt a description, nor shall we copy the claim, but merely observe that the machine is what it purports to be, a real improvement upon that formerly patented. We have spoken of it as complex, but we do not think it unnecessarily so; it performs its work well, does not appear liable to derangement with ordinary care, requires but little address in its management, and reflects a very high degree of credit upon the inventors, who have manifested a rare degree of skill in the whole arrangement.

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18. For improvements in the *mode of manufacturing Wooden Screw Bedsteads;* Palmer Williams, Towanda, Bradford county, Pennsylvania, February 16.

The bedstead rails are to have right and left handed screws on opposite ends, and corresponding female screws are to be cut in the posts; but the patent is obtained for the machinery by means of which the screws are so cut that they will come correctly to a shoulder at each end, when they are screwed into the posts. The apparatus for cutting the screws does not differ from that known to workmen, and the machinery employed merely confines the posts whilst being cut, so that the proposed effect will be produced.

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19. For an improvement in the manner of constructing *Bench and other Vices;* Linus Dean, Utica, Oneida county, New York, February 16. (See Specification.)

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20. For an improved mode of *Holding back a sled on icy descents;* James Andrews, Camden, Waldo county, Maine, February 16.

On the back end of each runner of the sled there is to be a hinged clip, which embraces the runner on each side, and has on its back end a kooked point, bending downwards, which is capable of being forced into the ice when descending a hill; for the purpose of forcing it down there is a screw above each of the hold-backs, which may be turned at pleasure, and made to act upon the clip. The claim is to "the whole of the aforesaid irons, fastened and used as aforesaid."

We apprehend that the foregoing patent will be of little value, as the devices for holding back vehicles upon runners, when descending hills covered with ice are no novelties, and may be used in various ways without interfering with the particular plan above described.

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21. For an improved *Balance for Weighing;* B. Morrison, Milton, Northumberland county, Pennsylvania, February 16.

The apparatus is denominated "The Druggists' Balance," and is a modification of that kind which has the scale above the beam, many of which were imported from England a few years since, having been patented in that country. Mr. Morrison obtained a patent for a balance somewhat similar to this in February 1833, and also in December of that year. (See Journal vol. XII,. p. 93, and vol. XIV., p. 30.) The present plan differs from the former in some points which appear to possess decided advantages, but it is not thought necessary to give a long verbal description of an arrangement which could not be rendered clear without a drawing.

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22. For improvements in the process of *Preserving Timber for various purposes;* Webster Flockton of Great Britain; residing in Spa road Bermondsey, Surry county, Near London, February 16. (See Specification.)

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23. For a *Machine for Cutting Furs from Skins;* Samuel Johnson, Walnut Lane, Surry county, North Carolina, February 24.

The cutting in this machine is effected by shears resembling tinmen's shears, with the fulcrum at one end; these are made to vibrate by means of a crank and pitman. The skin from which the fur is to be cut passes around and between feeding rollers, which present it to the shears. The claim is to "the method of vibrating the upper shear on a pivot at one end, and regulating the same by the springs bearing against the pitman, as described, and the board for preventing the skin from rising, in combination with each other, and with the other parts of the machine as described."

There does not appear to be much novelty in the plan of the foregoing machine, but the patentee has, in his claims, limited himself to certain particular combinations, which are deemed new.

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24. For *Propelling Machinery by means of Electro-Magnetism;* Thomas Davenport, Brandon, Rutland county, Vermont, February 25. (See Specification.)

Specifications of American Patents.

Specification of a Patent for Combined Elliptical and Spiral Springs; granted to William Croasdale, *Hartsville, Bucks county, Pennsylvania, February 3d,* 1837.

To all whom it may concern, be it known, that I, William Croasdale, of Hartsville, Bucks county, Pennsylvania, have invented an improvement in the construction of the Eliptical, or double bow springs for carriages of various kinds, and I do hereby declare that the following is a full and exact description thereof.

The springs are made, and put together in the usual manner, my improvement consisting of a spiral spring, or springs, which I interpose between the two bows of the elliptical spring, and sometimes between the ends of the elliptical springs and the axle, and between them and the under side of the carriage. In most cases I deem a single spiral spring sufficient, and this I make of steel wire, of from one-eighth to three-eighths of an inch in diameter, twisted with the coils open, and placed in the middle between the two bows, so as to extend from one to the other; the ends of the wire entering into holes prepared to receive them.

I generally wind these springs with the coils each of the same size, when the outline will be cylindrical, but sometimes I wind them so that their outline may be that of a double cone joined at their smaller ends, and the coils then pass more or less the one within the other, when pressed closely together. The outline may also be conical, or the spring may be bent in a zig-zag form, or otherwise, but I deem the spiral best.

If preferred, there may be spiral springs placed under the elliptical springs, their lower ends bearing on the axle-tree; and sometimes also above the ends of the elliptical springs, extending up to, and bearing against, and a suitable piece of timber attached to the under side of the carriage, or vehicle; thus there may be four such springs without, whilst there may be two or more such springs placed at suitable distances apart within, the elliptical springs, so graduated as to adapt them to the double bow. The spiral springs thus placed, give greater elasticity and liveliness to the elliptical spring, and also tend to prevent its breaking.

What I claim as my invention, and wish to secure by Letters Patent, is the adding to, and combining with, the elliptical, or double bow springs, one or more spiral springs, in the manner and for the purpose herein set forth. WILLIAM CROASDALE.

Specification of a Patent for rendering Boots and Shoes Water-proof; granted to PATRICK G. NAGLE, *of the City of Philadelphia, February 10th,* 1837.

Be it known that I, Patrick G. Nagle, of Philadelphia, in the County of Philadelphia, and State of Pennsylvania, have discovered a new and useful composition for rendering boots and shoes water-proof, which is described as follows.

Take two pounds of balsam of capivi, five pounds of the essence of the myrtle tree, one pound of gum copal, two pounds of rosin, three pounds of rendered suet; cut all the hard subtances into small particles. Put the whole into a large vessel, and let the admixture boil for a few minutes until the ingredients become well incorporated together, and converted into a paste.

Mode of Application.—When the boots are crimped, and while wet on the crimping-boards, lay on the paste (it being warm at the time of applying the same;) in this state it will penetrate every part of the leather as it dries.

The above is for the leg part of the boot.

The following is for the bottom part of the boot.

Take the above in a dissolved state, or a part of it, and apply a sufficient quantity of rosin so as to make it into a thick paste (the rosin to be melted.) Then lay it on in the usual way in which the workman puts on paste after sowing on the welt. In this manner the inner sole becomes prepared to resist water as well as the outer sole.

The discovery claimed by the subscriber, and by him desired to be secured by Letters Patent, consists in the before described composition for rendering boots and shoes water-proof. PATRICK G. NAGLE.

Remarks by the Editor.—Numerous mixtures of wax, resins, and fatty matters have been employed for the purpose of rendering leather water-proof, and recipes of this kind are common in the journals, and other works In that given by the patentee there is some obscurity, and some empiricism

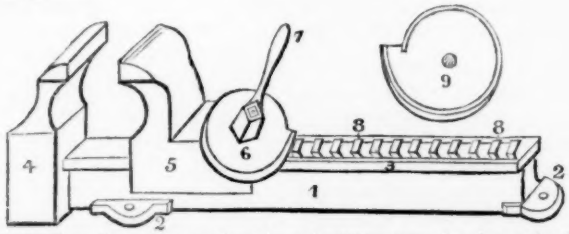
We do not know what is intended by "essence of the myrtle tree," but suppose that myrtle wax may be the thing designed; if so, it is a novel kind of essence. We apprehend that common turpentine would answer all the purposes intended to be answered by the capivi, (copaivi) and that in actual practice it is substituted.

The directions for saturating the sole are rather obscure; what is intended by "taking the above in a dissolved state, or a part of it," not being very apparent. The patentee has an undoubted right to the composition which he has claimed, but to this he is confined, and if it is superior to any of the analogous mixtures which have been used, or which may be readily made without infringing his rights, he has a better thing than we believe he has.

Specification of a Patent for an improvement in the manner of constructing Bench, and other Vices; granted to LINUS DEAN, *City of Utica, State of New York, February 16th*, 1837.

To all whom it may concern: be it known, that I, Linus Dean, of the City of Utica, in the county of Oneida, and State of New York, have invented a new and improved mode of constructing Bench, and other Vices; and I do hereby declare that the following is a full and exact description thereof.

In my improved vice, the movable jaw is not made to open by means of a joint pin, in the ordinary manner, but it is provided with grooves on its lower side, which fit on to, and by means of which it slides freely along a horizontal bar, the upper edge of which projects over on each side so as to form tongues, or ribs, which fit into the grooves of the sliding jaw. Upon one end of this horizontal bar the fixed jaw is situated, being firmly attached thereto. The bar has flanches on its lower side, by which it is to be attached to the bench. Instead of moving the sliding jaw by a screw, I effect this by means of a rack and a spiral, or volute, wheel, the fillet of which takes into the teeth on the rack, which teeth occupy the upper side of the sliding bar.



1, Bar; 2, flanches; 3, tongue; 4, fixed jaw; 5, sliding jaw; 6, volute wheel; 7, handle; 8, rack; 9, inside of volute wheel.

The spiral, or volute wheel revolves upon a pin rising from the sliding jaw, near to its back end; the spiral, or volute, is formed upon the lower face of this wheel, making one entire turn thereon. The whole is inclined back from a horizontal position, to enable the fillet, or spiral, to take into the rack teeth; a projecting pintle on the upper side of the wheel receives a handle, or wrench, by which it may be turned, which pintle forms a socket in the centre of the wheel to receive the pin on which it turns. The size of the respective parts will, of course, depend upon the size of the vice itself. The following are the general dimensions of the principal parts of one which I have made. The bar is about two feet in length, two and a half

inches deep; and one and a fourth inch wide. The fixed jaw is about the size, and is in the form of, that of an ordinary smith's vice; the movable jaw is of the same size and form, but it has a projecting piece extending back about six inches, which forms a part of the cheeks of the slide, and from which rises the pin upon which the spiral wheel revolves; the wheel is about six inches in diameter, the thread, or fillet, forming the spiral, about five-eighths of an inch square; and the pin which passes through the wheel, two inches high, and one inch and a half in diameter. The bar of this vice is to be secured upon the bench, with the fixed jaw towards the workman.

Having thus fully described the construction of my improved vice, I do hereby declare that I do not claim to be the inventor of the sliding jaw, this having been before made and used, but it has hitherto been moved, as in the common vice, by means of a screw. What I claim, therefore, as constituting my invention and improvement, is the substituting the rack and spiral wheel for the screw, as herein set forth, and arranging the auxiliary parts substantially in the manner described. LINUS DEAN.

Remarks by the Editor.—When we first examined the description and drawing of this vice, we were impressed with the idea, that the mode of opening and closing it would prove inconvenient if not otherwise objectionable, but we have since seen and carefully examined a number of them with much satisfaction. The spiral wheel operates not only well, but pleasantly. The extent to which the jaws may be separated is very great, and in all situations they preserve their parallelism, the convenience of which is at once apparent. By raising the spiral wheel the sliding jaw may be slid along, and much time in opening and closing be thus saved. In fine, we think the contrivance not only possesses the merit of originality, but the more important one of real utility, for many purposes.

Specification of a Patent for improvements in the mode of Preserving Timber for various purposes; granted to WEBSTER FLOCKTON, *of the kingdom of Great Britain, February 16th*, 1837.

To all whom it may concern: be it known, that I, Webster Flockton, of Bermondsey, in the county of Surrey, in the kingdom of Great Britain, Merchant, have invented improvements in preserving Timber, or Wood, for various purposes. My invention consists in impregnating timber, or wood of various descriptions, with a metallic solution, whereby such timber or wood will be preserved: and in order to produce the necessary solution, I saturate the essential oil of vegetable tar with the oxide of iron, which I consider the best and cheapest means of carrying my invention into effect, though I do not confine myself to the precise means hereafter described, as variations may be made; my object being to impregnate timber, or wood, for any purpose to which it may be found applicable, with a metallic oxide as above stated and as is more fully described hereafter.

But in order that my invention may be fully understood and carried into effect, I will describe the method I pursue of combining such materials together and applying them to the purposes aforesaid. I take a quantity of tar (either Stockholm, Archangel, or American) which I submit to the process of distillation, and the apparatus, or still, which I use for this purpose is similar to what is called a pitch still, which is made of copper, and well known, and forms no part of my invention, nor does the process of distilla-

tion for separating the essential oil from tar, which is effected in the manner following. The still which I use will contain about 400 gallons, but I do not put into it more than three-quarters of that quantity of tar, or twelve barrels, of the usual size, of either of the kinds before mentioned. The first product will be the acid of the tar, bringing with it a light-coloured essential oil which separates immediately and floats upon the surface of the acid in the receiver, which I *prefer of wood* (a cask with one head furnished with a cock for the withdrawing the acid from below, being applicable to the purpose;) after some time the acid will cease and the essential oil will come over in a very considerable stream which I collect from the receiver to the extent of about four gallons to the barrel, or forty-eight gallons in the whole, including that which came over in the first instance with the acid; the fire is then to be withdrawn and the contents of the still, which by the extraction of the essential oil has become *pitch*, allowed to remain *in the still* until the following morning to cool, then it may be let off by means of a pipe fitted with a brass or iron plug, into a large receiver of cast-iron, or other suitable material, and finally put into casks for sale.

I will now proceed to describe the combining of the essential oil with the other materials for the making of my "Metallic Solution." To effect this, I place *two*, or more, large casks upright, removing the upper head of each, and throw into them well rusted iron hoops, or tin cuttings. I then pump into them one hundred gallons, or more, of the essential oil of tar, before described, completely covering the metal. This oil I cause to be repeatedly pumped every day from one cask to the other for about six weeks, by which time the oil will have become very black and much increased in gravity, whilst the iron hoops, or tin cuttings will appear quite bright and free from oxide.

They are then to be taken out and piled up in an open space of ground and set fire to, for the purpose of burning off the oil, and afterwards laid by for re-oxidation, which may be much facilitated by pouring over them a weak solution of common salt and water; when they have again become rusted they are fit for use.

I will now proceed to describe the method I pursue in saturating timber and wood with the metallic oxide.

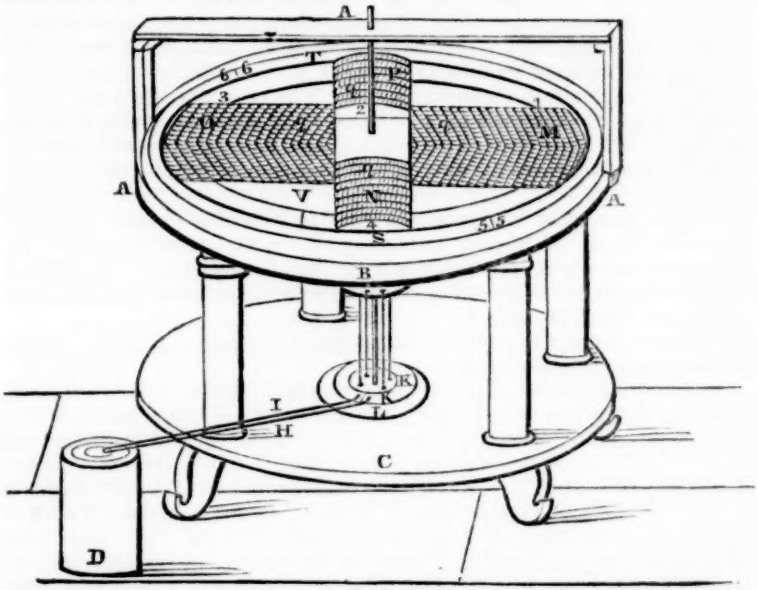
For saturating piles *already* driven into the sea, forming jetties, or piers, I cause an inch auger to be passed down the centre of the piles to the bottom end, if possible, or as far down as can conveniently be done, and the "liquid oxide" poured down the hole until filled. This is to be repeated as often as may be thought necessary, but, generally, in two or three days it will be found oozing through the pores of the wood, depositing an incrustation of *Iron*, which, in combination with the *Essential Oil* of the *Tar*, resists alike the action of the water and the attacks of the worm. A wooden plug or tree nail is then to be driven fast into the hole, which may be removed by the auger at any time for the purpose of giving the piles a fresh supply. This method is likewise applicable to the timber used in blocking streets, the wood work of railways, and to all wood subject to damp, or moisture, or the attacks of worms, or other vermin.

For out-door buildings liable to dry rot, it may be used cold in the usual way of varnish, or tar, with a brush; for being perfectly liquid it penetrates most rapidly, drying completely in eight or ten hours, when a second dose may be given. Paint applied afterwards dries quickly, but for most purposes two or three applications of the preparation render any other coating unnecessary; for as soon as the pores of the wood become filled, it assumes

the appearance of varnish. Having thus decribed the nature of my invention, and the manner of carrying the same into effect, I would have it understood that I lay no claim to any of the materials *separately*; and it will be evident that the means of carrying the same into effect may be varied to suit the particular object to which the invention may be applied; but I would have it understood that what I claim is the impregnating timber, or wood of various descriptions, with the metallic solution above described, whereby such timber or wood will be preserved. WEBSTER FLOCKTON.

Specification of a Patent for the application of Electro Magnetism to the propelling of Machinery; granted to Thomas Davenport, of Brandon, Rutland County, Vermont, February 1837.

Be it known, that I, Thomas Davenport, of the Town of Brandon, in the County of Rutland, and State of Vermont, have made a discovery, being an application of magnetism, and electro-magnetism, for propelling machinery, which is described as follows, reference being had to the annexed drawings of the same, making part of this specification.



The machine for applying the power of magnetism, and electro-magnetism, is described as follows: The frame A may be made of a circular, or any other figure, divided into two, or more platforms, B and C; upon which the apparatus rests, and of a size and strength adapted for the purpose intended. The galvanic battery, D, is constructed by placing plates of copper and zinc, alternately, of any figure, in a vessel of diluted acid: there are two conductors, H and I, one from the copper and one from the zinc, in the vessel D, leading to, and in contact with copper plates, K and L, placed upon the lower platform. These plates, or conductors, are made in the form of a segment of a circle, corresponding in number with the

artificial magnets hereinafter described; they are placed around the shaft, detached from one another and from the shaft, having a conductor, leading from the copper plate of the battery, to one of said plates on the lower platform, and another conductor leading from the zinc plate of the battery, to the next plate on said lower platform, and so on alternately (if there be more than two plates on said lower platform) around the circle.

The galvanic magnets, M, N, O, P, are constructed of arms, or pieces, of soft iron in the shape of a straight bar, horse shoe, or any other figure, wound with copper wire *q*, first insulated with silk between the coils: these arms project on lines from the centre of a vertical shaft R, turning on a pivot, or point, in the lower platform; said copper wires, *q*, *q*, extending from the arms parallel, or nearly so, with the shaft down to the copper plates, K and L, and in contact with them.

The galvanic magnets, are fixed on a horizontal wheel of wood V, attached to the shaft.

The artificial Magnets S, T, are made of steel, and in the usual manner. They may be of any number, and degree of strength, and fixed on the upper platform, being segments of nearly the same circle as this platform; or if galvanic magnets are used, (which may be done,) they may be made in the form of a crescent, or horse-shoe, with their poles pointing to the shaft.

Having arranged these artificial magnets, on the top of the upper circular platform, there will be a corresponding number of magnetic poles—the north marked 5, and the South pole 6. Now we will suppose the machine to be in a quiescent state; the galvanic magnet, No. 1, being opposite the south pole of the artificial magnets, the galvanic magnet, No. 3, will, of course, be opposite the south pole, No. 6, and the galvanic magnets, No. 2 and 4, will be opposite each other, between the poles just mentioned.

There being a corresponding number of copper plates, or conductors, placed below the artificial magnets around the shaft, but detached from it, as well as from each other, with wires leading from the galvanic magnets to these plates, and in contact with them, as before described, these wires will stand in the same position, in relation to the copper plates, that the galvanic magnets stand to the artificial magnets, but in contact with the plates.

Now in order to put the machine in motion, the galvanic magnet, No. 2, being charged by the galvanic current passing from the copper plate of the battery, along the conductors and wires, becomes a north pole, whilst, at the same time, the magnet, No. 4, is charged by the galvanic current passing from the zinc plate of the battery, and becomes a south pole; of course the south pole of the artificial magnet, No. 6, will attract the north pole of the galvanic magnet, No. 2, and will move it a quarter of a circle; the south pole of the galvanic magnet, No. 4 being at the same time attracted by the north pole, No. 5, causes the said magnet, No. 4, also, to perform a quarter of a circle: the momentum of the galvanic arms will carry them past the centres of the poles, No. 5 and 6, at which time the several wires from the galvanic magnets, will have changed their positions in relation to the copper plates, or conductors:—For instance, the north pole, No. 2, having now become a south pole, by reason of its wires being brought in contact with the conductors of the zinc plate, and No. 4 having, in like manner, become a north pole, its wire having changed its position from the zinc plate to the copper plate, the poles of the galvanic magnets are, of

course, now repelled by the poles that before attracted them; and in this manner the operation is continued, producing a rotary motion in the shaft, which motion is conveyed to machinery, for the purpose of propelling the same.

The discovery here claimed, and desired to be secured by Letters Patent, consists in applying magnetic and electro-magnetic power, as a moving principle for machinery, in the manner above described, or in any other substantially the same in principle.

THOMAS DAVENPORT.

Remarks by the Editor.—The subject of the forgoing specification is one of great interest, and it has arrested a corresponding portion of public attention; we are likely soon, therefore, to have the question solved, whether this new power can be advantageously applied to the propelling of machinery as a substitute for the steam engine. Most of our readers, it is presumed, have seen Professor Silliman's notice of Mr. Davenport's machine, published in the Journal of Science, in April last, which contains much information respecting the attempts which had been made for the producing of motion by electro-magnetic apparatus. Since that period, advice has been received from Europe, showing that experiments upon this subject are in progress under the direction of some of the most distinguished Philosophers in various portions of that quarter of the globe.

We do not know by whom, or at what date, the first successful experiment of producing a direct rotary motion, by the electro-magnetic apparatus, upon a principle analogous to that upon which Mr. Davenport has proceeded, was performed. As early, however, as June, 1833, an article appeared in the London Mechanics' Magazine, proposing such an apparatus, and giving a figure of one which it was supposed, would answer the purpose; a supposition which was, undoubtedly, well founded. Not long after this, Mr. Saxton, we believe, produced a rotative machine by electro-magnetism, but we are not informed respecting its particular arrangements.

The history of the production of the machine patented by Mr. Davenport, is a history of the successful efforts of an individual, who to an indomitable perseverance, must have superadded, extraordinary natural abilities. His business is that of a blacksmith, and his advantages in point of education were not greater than usually falls to the lot of persons in country places, engaged in such pursuits. Accident brought to his notice, one of Professor Henry's electro-magnets, which he eagerly purchased, under a conviction that he could render it available as a motive power; this was in the year 1833, and in July 1834 he had so far succeeded as to produce a rotative machine, and this he effected in a country village, unaided by scientific knowledge, by books, or by the encouragement of men of superior attainments, or with kindred spirits. Whatever may be the final result of his labours, his merits are of a high order, and he has proved himself well worthy of the most splendid success. Should his machine finally accomplish that which he and many of his friends anticipate, its value will be incalculable, for although he may have been superceded in Europe, his claim as inventor will undoubtedly prove valid in his own country, and ambition need not carry him beyond it. We have twice seen his machine in operation, formerly in New York, and recently in Washington, where it was exhibited to the President, and the Heads of Departments. So far as the evidence of a model is to be taken, its performance is quite satisfactory; and Mr. Davenport is now occupied in constructing one which is intended to drive a Napier Press, re-

quiring a two-horse power. This, should it succeed, will be a fair test of its value, and we confess that, although our expectations do not generally partake of the sanguine in such matters, not only our hopes, but we may say our confidence, has increased as we have become acquainted with the progress of the experiments which are being carried on.

We are well aware, that should it be eventually proved, that an available power may be obtained, which may be substituted for that of steam, its adoption would depend entirely upon its economy, with respect to which we cannot have satifactory data until a machine of several horses power shall have been produced; the probability, however, is, that the cost of operating the electro-magnetic apparatus, will be much below that of the steam engine.

Progress of Practical and Theoretical Mechanics and Chemistry.

On the use of Steam in the Economising of Fuel. By Andrew Fyfe, M. D., &c. Edin.

Dr. Fyfe caused steam to pass through a porcelain tube, stuffed sometimes with charcoal, sometimes with coke heated to redness in a furnace, collected the resulting gas, generally over a water trough, but sometimes over mercury. His inference from a variety of experiments is, that the gas contains hydrogen, oxygen, and carbon, and that the two last are in the state of carbonic oxide. Hence the combustion of the gas gives rise to the formation of carbonic acid and water.

When air was freely admitted to the incandescent material, at the same time that steam was driven through it, Dr. F. found that the water of the steam was, in part at least, consumed, and that the heat was thereby augmented. This was proved by the greater quantity of water evaporated in a given time. On an average, for each ounce of steam thrown into the furnace, there were four ounces additional evaporated, over and above that evaporated without the transmission of steam, provided the steam was thrown in cautiously. This increase of temperature, by the use of steam as a fuel was not effected at the expense of a greater quantity of fuel, for there was rather less fuel consumed when steam was transmitted through it than when omitted, while at the same time the quantity of water evaporated was increased. To arrive at correct results, it is necessary to throw the steam in cautiously.

It is thus proved that water, while passing in the state of steam through fuel, not only acts as a sort of blast, but, at the same time, itself undergoes combustion, by the formation and consequent consumption of inflammable gaseous products; and the increase of heat, Dr. F. thinks, will more than compensate for any extra expenditure for converting the water into vapour.

The author states that the only instance which he had found on record in which steam was passed through fuel with any definite object in view, is mentioned by Mr. Mushet in the 6th volume of Tilloch's Magazine. Had he consulted the early volumes of Silliman's Journal, and also the 25th vol. of that work, he would have found a succession of papers by Samuel Morey, in some of which the use of water as a fuel is distinctly alluded to, and in the last paper its application is proposed by that indefatigable experimenter to one or two practical purposes.

The foregoing account of Dr. Fyfe's experiments is abstracted from his papers in Jameson's (Edinburgh) Journal, No. 45. G.

Bunnett's Revolving Iron Safety Shutters.

There is scarcely any thing more desirable in populous towns and cities, than a ready and convenient method of securely closing the houses and shops of the inhabitants. The modes that have been devised for effecting this purpose are very various; but up to a comparatively recent period, most of the plans in common use were more or less inconvenient—many of them excessively clumsy, both as regarded the material and the manner of its application. The trouble of putting up every night, and taking down every morning, from ten to twenty heavy wooden shutters, together with the difficulty of finding a suitable place for stowing them away in the day-time, were among the most prominent evils entailed upon shopkeepers by the old-fashioned methods.

The cellar was very often the only eligible place of depository, from whence the shutters were drawn out across the foot-pavement of an evening, to the great and grievous annoyance of the pedestrians, with which the thoroughfares are at that time crowded.

Sundry remedies have been proposed for these evils, and of several improved shutters that have appeared, the most recent, and, in my humble opinion, the best, are Bunnett's Revolving Iron Safety Shutters, for which his Majesty's royal letters patent were obtained in June last. Although these shutters bear a slight resemblance to another invention for a similar purpose, they are essentially different in a most important particular.

Bunnett's shutters consist of a series of narrow plates or strips of iron, placed horizontally, and connected together by hinges of a simple construction, rivetted on to their under surface, in such a manner, that the iron plates overlap each other, and entirely conceal the hinges from external observation; considerable advantage arises from this circumstance, inasmuch as the hinges cannot be seen on the outside of a building; they are therefore not liable to be cut, nor can the knuckle-pins of the hinges be removed. The pieces of metal to form the hinges are struck out of plate iron, and afterwards bent up into the required form; each hinge being held to the strip forming the shutter by three rivets. The knuckles of each hinge being bent in opposite directions, allows the strips of iron to overlap each other; at the same time, the combined shutter is capable of bending over and winding round the roller, to which the first set of hinges is fastened. This arrangement entirely obviates the necessity for cutting away any portion of the substance of the shutter itself.

A front view of a window closed with one of Bunnett's patent shutters, has a light and elegant appearance, closely resembling a Venetian blind, the resemblance being greatly increased when they are painted (as they should be) green.

The following method is adopted for opening and closing these shutters. An iron roller with a toothed wheel at one end, is fixed in suitable bearings on each side at the top of the window; a worm, or endless screw on the top of a vertical shaft, takes into this toothed wheel, while the lower end of the shaft carries a small bevelled wheel, working into another similar wheel placed at right angles with it. The axle of the last bevelled wheel is squared off for receiving the handle, the turning round of which raises or lowers the shutter.

The compact form of these shutters requires no deviation from the present mode of building, but that of placing the lintel over the window a few inches higher than usual, to admit the shutter when rolled up.

The same principle may also be advantageously resorted to in fitting up valuable libraries; it is equally well adapted to the front of glass cases, &c. containing jewellery, or other valuables. As a temporary partition, for the division and protection of shops, the iron shutters will be found superior to every other contrivance.

The ends of the shutter slide up and down in two metal grooves, one on each side of the window-frame; there are two steady-pins in the window sill, which fit into corresponding holes in the lowest bar of the shutter; on the inside of this bar, one or more square holes are made for the reception of self-acting spring-bolts, which catch the shutter on its descent, and most effectually prevent its being raised by any force either internal or external, until they are withdrawn. These bolts may be replaced by locks, where the shutters are not to be opened but in the presence of the owner.

Among many other advantages offered by this ingenious invention, the following are some of the most striking:—Perfect security in all situations against both fire and thieves; the simplicity of construction, and the nature of the materials employed, which ensure their continuance in good and efficient working order; the short space of time required to open or close them (a few seconds,) and the ease with which it is accomplished, enabling even females or children so manage them without any liability to accident. They are equally adapted to the house of business or private residence, the absence of inside shutters allowing the windows to be finished in any way that taste or necessity may dictate.

Another most important advantage would be gained by fixing these shutters to fire-places generally, so as to fall in front of the stove when fires are left, or extinguished for the night, and thereby preventing those accidents which frequently arise for want of such a provision. In the event of the chimney taking fire, the shutter would enclose the falling particles of ignited soot, and by stopping the draft, extinguish the fire, thereby removing the cause of danger.

Although the first year of Mr. Bunnett's patent is not yet expired, his shutters have already come into extensive employment. Wm. Baddeley.

May 25, 1837. Mech. Mag.

The Pine Apple Plant and its Fibre.

This plant, which has hitherto been valued solely as ministering to the luxuries of the table, has lately had a new interest attached to it from the discovery of a fibre contained in its leaves, possessing such valuable properties, that it will, in all probability, soon form a new and important article of commerce.

This fibre is found, on comparison, far to surpass in delicacy of texture, those materials which now form the basis of our woven manufactures: of these the principal are silk, wool, cotton, and flax. Silk is a continuous fibre, often extending, without interruption, to the length of 1000 feet; viewed under the microscope, it is found to be perfectly cylindrical, beautifully smooth, glossy, and transparent. The best kind of prepared silk varies from $\frac{1}{1700}$ to $\frac{1}{2000}$th part of an inch in diameter. Wool, on the other hand, is a rough cylindrical fibre, which appears as if plated with irregular scales, the edges of which overlap each other; though invisible to the naked eye, its roughness may be easily detected, by drawing a fibre between the fingers, in a direction from its end to its root. It varies greatly in both length and size, the diameter of very fine wool being from $\frac{1}{700}$th to

$\frac{1}{1200}$th part of an inch. Cotton, being the down of a seed-pod, has peculiar characters, a thin continuous tube, presenting the appearance of a flattened and twisted cylinder, arising, probably, from the compression which the fibre undergoes before the opening of the pod: its diameter is about the $\frac{1}{1000}$th part of an inch. Flax, in many respects, differs from the foregoing materials; a filament of silk, wool, or cotton, admits of no division, while, on the other hand, each filament of flax is a fasciculus or bundle of fine fibres, connected by a thin membrane, and the natural gluten of the plant. These fibres may be termed ultimate, as they admit of no further subdivision: they are cylindrical tubes, disposed parallel to each other, and having the appearance of innumerable joints; their diameter varies, in different flaxes, from $\frac{1}{700}$th to $\frac{1}{2300}$th part of an inch.

The fibres of the pine-apple plant are also disposed in fasciculi, each apparent fibre being an assemblage of fibres adhering together, of such exceeding delicacy, as only to measure from $\frac{1}{5000}$th to $\frac{1}{7000}$th part of an inch in diameter; viewed under the microscope, they bear considerable resemblance to silk, from their glossy, even, and smooth texture. They appear altogether destitute of joints, or other irregularities, and are remarkably transparent, particularly when viewed in water: they are very elastic, of great strength, and readily receive the most delicate dyes. This last fact appears singular, when we bear in mind the resistance, if we may be allowed the expression, which flax offers to dyes. With much trouble, and by long processes, flax will receive a few dark dingy colours: all light and brilliant ones it wholly resists, they do not enter the fibre, but merely dry upon it externally, and afterwards easily peel, or rub off,—in short, it may be said to be *painted*, and not *dyed*.

The preparation of the pine-fibre is exceedingly simple. If a leaf of this plant be examined, it will be found to consist of an assemblage of fibres running parallel from one extremity of the leaf to the other, embedded in the soft pabulum. All the process necessary is to pass the leaf under a "tilt hammer," the rapid action of which, in a few seconds, completely crushes it, without in the slightest degree injuring the fibre, which remains in a large skein, and then requires to be rinsed out in soft water, to cleanse it from its impurities, and be afterwards dried in the shade. So simple and so rapid is the process, that a leaf, in a quarter of an hour after being cut from the plant, may be in a state fit for the purposes of the manufacturer, as a glossy, white fibre, with its strength unimpaired by any process of maceration, which, by inducing partial putrefaction, not only materially injures the strength of flax, but also renders it of a dingy colour.

The pine-plant abounds both in our East and West India possessions, and may be easily propagated from the crown; the offsets from round the base of the fruit, which often amount to upwards of twenty in number; and from the young plants which spring from the parent stem; its cultivation requires but little care or expense, and the plant is of such hardy growth, as to be almost independent of those casualties of weather, which often prove so detrimental to more delicate crops—it is one of those plants which Nature has scattered so profusely through tropical regions, whose leaves are thick and fleshy, to contain a large supply of nourishment, and covered by a thick, glazed cuticle, which admits of so little evaporation, that many of the tribe will thrive upon a barren rock, where no other plant could live. From the arge portion of oxalic acid which the leaves also contain, no animal will ouch them, and they are, therefore, exempt from the tresspasses of cattle, &c.,indeed, no greater proof of the hardiness of the plant can be given,

than the fact, that in many places where lands have been under tillage, and have afterwards been abandoned, and allowed to return to a state of nature, the pine-apple plants form the only trace of former cultivation; every other cultivated plant has died away before the encroachments of the surrounding wood, while they alone have remained increasing from year to year, and have spread into large beds.

In adverting to the present state of our West Indian colonies, we cannot but think that the discovery of this fibre will prove to them a most valuable acquisition. The small amount of labour and of capital requisite for the cultivation of the plant, its hardy growth, its abundant produce, the facility with which the fibre may be prepared from its leaves, the trifling cost at which it can be brought to market, and the value attached to it, as an article of commerce, by those manufacturers to whom it has been submitted, seem fully to justify the opinion that it is calculated to open a new source of wealth to the colonies, and become a staple and valuable article of British manufacture.

Mag. Pop. Sci.

On the Preparation of Sulphuret of Carbon.

M. Mulder directs, that in an iron bottle in which mercury is imported, besides the hole which is already there, another should be bored near it. Into the first of these openings a copper tube $\frac{4}{10}$ inch diameter, bent twice at right angles is to be screwed, and into the second a straight tube, also of copper, is to be introduced. Then the bottle is to be filled with pieces of charcoal, recently heated to redness, of such a size that they can easily pass down the tube. After having firmly screwed in the straight and curved tubes, place the bottle in a furnace and heat it, after having closed the opening of the furnace with a stone cut in halves to prevent inconvenience to the operator from the ascending heat.

Adapt to the curved tube a Woulfi's bottle half filled with water and surrounded with a freezing mixture, and when the iron bottle is sufficiently heated, introduce by the straight tube fragments of sulphur and immediately close the mouth of the tube with a plug; the sulphur fuses, and falling upon, penetrates the pieces of charcoal, and when the sulphur is gradually added, but little gas is evolved and abundance of sulphuret of carbon obtained.—*Journal de Pharmacie*, Jan. 1837.

Lon. & Ed. Philos. Mag.

Researches made on the Bouquet of Wines. By Messrs. Liebig and Pelouze.

It has been long suspected that wine contained a peculiar principle which is the cause of the agreeable odour generally known as the bouquet of wines, which principle seems hitherto to have eluded all the attempts which have been made to detect it. "We have now," however, remarks the authors of this extract, "the honour of presenting to the Academy certain experiments which we have made on an essential oil, which was sent to us by M. Deleschamps of Paris, and which, from all its properties, appears to be the principle so long sought after. This substance has an exact resemblance to the essences, and was given to us as such; its odour is completely that of old wine, with the exception of its intensity. Its chemical properties remove it from the class of essential oils, and regarding its constitution it sheds a new light on organic chemistry, in supplying the first well established example of the existence of a true ether, which is produced in the act of fermentation, and without the aid of the chemist. This ether is composed

of an atom of sulphuric ether, and an atom of a new acid which we propose to designate *œnanthic* acid, and which is formed of $C^{14} H^{26} O^2$. The condensation of its vapour is that of formic and acetic ethers. The oil presents itself under the form of a greasy oil which crystallizes at 56° Fahr. In its free state it contains one atom of water, which it loses by distillation. By combining it with sulphuric ether, we can easily reproduce *œnanthic ether*. We have deemed it necessary, for the purpose of establishing the composition of one of the most remarkable of these acids, viz. the *melitic acid*, to make some analytic investigations concerning its combination with oxide of silver. We believe that this acid may be regarded as a hydrous acid, and our experiments on this point confirm the views of M. Dulong respecting oxalic acid. The melitate of silver dried in vacuo, by the side of sulphuric acid, contains some hydrogen, which it loses only at the temperature of 356° Fahr. under the form of water, and with change of colour. There is not one other salt of silver which exhibits the same result; all the others are anhydrous. In this particular instance, the formation of water appears to be the consequence of the reduction of the oxide, and not a simple volatilization of water which had pre-existed in the salt. According to all known analyses, the melitic acid contain three atoms of oxygen. We have reason to think it contains four, and besides two atoms of hydrogen; and that this hydrogen enters into the constitution of all the melitates except into that of silver heated to 356°, so that this last represents a combination of metallic silver, with the radicle of hydrous acid." Ed. New Philos. Jour.

Hot Air Blast.

The benefits which this new process appeared to promise, and the sanguine expectations of economy and profit that have been entertained, do not appear to be fully realized. Still it is not without its advantages, and with respect to the use of anthracite coal in the reduction of iron ore, the application of the hot blast has evidently afforded, under the management of Mr. Crane, in Wales, the most encouraging indications of a success, which in its application to our own mines of coal and iron may prove of incaluable importance.

The following extracts from a communication of W. Wood to the London Mining Journal, point out what he deems to be the causes of failure in the use of hot air in the smelting of iron, and also the incidental benefits which it already produces. It is much to be desired that experimenters and iron masters may not be too soon discouraged by the unfavourable nature of these statements, for it is very possible that the means may be hit upon of obviating the causes which appear to occasion a deterioration of the quality of the iron, and still secure to the hot air process all the facilities and advantages to which both science and fact most evidently give it a claim. G.

The use of heated air in smelting, has greatly facilitated the production of grey iron, and has, in many instances, occasioned a material increase in the make; but that it has been productive of the great advantages which were at first supposed to accrue from its use, has been by no means established. I do not mean to dispute, that at some concerns, a reduction in the quantity of fuel made use of may have been effected, but that this has been the general result, I believe I may safely say, is by no means the case; on the contrary, in the majority of the works where it has been made use of, the saving effected is a mere bagatelle—the aggregate quantity consumed

in smelting, and heating the air, being very nearly equal to that which had been previously used in smelting with cold-blast. From all I can collect from different parties who have adopted the system, the only advantage they derive from it is, that the furnaces work more uniformly, not being liable to the fluctuations which are experienced when cold-blast is made use of. This is undoubtedly an advantage of considerable importance, and would be sufficient to recommend its universal adoption, were there no disadvantages connected with its use, which more than balance all the benefits derived from it, and must permanently operate against its general introduction. What I allude to is, the reduction of the actual strength of the iron produced by this process. That this is a fact, is now established beyond a doubt, by numerous experiments by different parties. I sometime ago instituted a series of experiments myself, on the relative strength of iron produced by the old and new process. The bars I made use of were one and one-eighth square, and supported at each end on supports two feet distant from each other; from the centre I suspended a scale board, on which weights were placed, and increased seven pounds at a time, until they broke. The bars were obtained from a neighbouring iron works, and were produced from the same materials, one set from cold-blast, and the other from hot, and were to all appearance of precisely the same quality, so much so that there was not the least apparent difference in the quality of each on inspecting the several fractures. The averaged result was, that the cold-blast iron broke under a weight of 2009 lbs., and the hot-blast iron under a weight of 1568 lbs. This shows a deficiency of more than twenty per cent. in the strength of the hot-blast. The deflection under equal weights was rather in favour of the hot-blast iron, proving it to be less flexible until it arrived at the breaking point. This is a most serious objection to such iron, and operates against its safety, when applied to purposes requiring great strength.

I have never seen any satisfactory reason assigned for the deficiency which is evident in its strength; it, however, in my opinion, admits of a solution on chemical principles. Sir Humphrey Davy was the first person who established the probability of the different earths being oxydes of metals, reducible to the metallic state in the same manner as the ores of other metals; and Berzelius succeeded in forming an alloy of silicium and iron, thereby establishing the fact of their affinity for each other. That the earths, in combination with the ironstone and other materials made use of in the blast furnace, are in small proportions reduced along with the oxyde of iron, there cannot be a reasonable doubt, and is in a measure proved by the analysis of pig-iron, which is invariably found in combination with alumine, lime, and silex, in various proportions. These there is every reason to believe, are in combination with the iron in a metallic state, thereby forming an alloy, whose force of cohesion is much less than that of pure iron, and in proportion to the quantities of these earths revived will its strength be affected. I am the more confirmed in this opinion by the result of an analysis which I made some years ago of iron produced from ironstone and from refinery cinder, the former producing much stronger iron than the latter. This analysis showed that there were three per cent. of earthy matter in the stronger iron or that produced from ironstone, and five per cent. in the weaker or that produced from refinery cinder. Both samples were smelted at the same iron works, with the same coal and limestone, and under precisely similar circumstances, therefore the fair inference to be drawn from the analysis is, that the earthy matter, in a metallic state, was the primary cause of the de-

ficiency in the strength. The predominant earth in each case was silex, from which we may infer that this earth has a weaker affinity for oxygen than alumine and lime, and is consequently more readily reduced to the metallic state than either of the others. That the affinity of silex for iron is strong, is proved by the circumstance that the refining processes made use of to produce malleable iron, do not entirely remove it, for it is found in the finished bar, in quantities bearing a proportion to that found in pig-iron, although of course in smaller quantity.

By the old process, the temperature of the furnace is much reduced by the introduction of the immense quantity of cold-air required to carry on combustion, and which must necessarily abstract a great quantity of caloric to elevate it to the temperature of the mass of matter in the furnace, and that, in the vicinity of the twyers, where intense heat is most required, for the purpose of producing perfect fusion of the earthy part of the materials, to effect a thorough separation of the metal from them. By the new process of heating the air previous to its introduction, its refrigerating effect is, in a great measure obviated, the temperature is kept up, and preserved in that state best calculated to increase the affinities of the oxygen in the ore, and of the iron when reduced to a metallic state, for the carbonaceous matter with which it is in contact, thereby facilitating the combination of a larger portion of carbon with the iron than is effected with cold-blast. If the furnace, by this process, is kept in a state better adapted to reduce the oxyde of iron, it also stands in the same position as regards the earths, and there cannot be a reasonable doubt as to the fact of their being revived in larger proportion than under the cold-blast system; and to this cause, I am persuaded, is to be attributed the deterioration in the quality as far as its strength is concerned. To satisfy myself on this point as far as possible, I analysed the iron, the experiment on the strength of which I have before noted, the result of which goes far to confirm this opinion, and is as follows:

From 100 parts of cold-blast iron I obtained

Silex	2.33	or Silicium	1.165
Alumine	0.78	or Aluminum	.545
Lime	0.26	or Calcium	.186
	3.37		1.896

And from 100 parts of hot-blast iron

Silex	4.27	or Silicium	2.135
Alumine	1.10	or Aluminum	0.767
Lime	0.41	or Calcium	0.399
	5.78		3.301

These analyses correspond very nearly with those before mentioned, and, in my opinion, leave little doubt as to the inferior quality of the iron produced with hot-blast being attributable to the same cause as that which operates against that produced from refinery cinder, which has hitherto baffled every attempt to improve its quality. The cinder here alluded to, is that produced in the first process, made use of in converting cast into malleable iron, which is technically termed, refining. That produced in the subsequent processes, makes iron of a much better quality, and in many cases a judicious mixture of it, improves the quality of that produced from ironstone.

I have not had an opportunity of fairly trying the effect of hot-blast on the strength of malleable iron, on a sufficiently extensive scale to speak decidedly

of it from my own knowledge, but from information I have received, from a person of good practical knowledge, and recently employed as a superintendant over a part of the manufacturing department of an extensive iron works, where the principle is applied to one furnace, I have every reason to believe that the effect is equally injurious to bars as to cast-iron; at any rate, as far as regards the iron produced at this particular works, and where that produced with cold blast is generally good. What this person states is, that the iron produced there is excessively weak and coldshort, so much so that a very small proportion of it can be made use of, mixed with the other iron. This is precisely the character of refinery cinder iron, to which it is in most respects analogous.

Circumstances may probably operate in some situations to modify the character of it, but whether sufficiently to make it desirable to adopt it, is what I am not at present prepared to speak to, experience alone can decide this point. I am, Sir, your obedient servant,

Abersychan Iron Works, Pontypool, W. Wood.
Aug. 5, 1837. Min. Jour.

Iron Trade in Scotland.

It appears from a statement of Bald and Geddes, mining engineers, that it is about seventy years since iron-works were erected in Scotland, at Carron, in Stirlingshire, and for twenty years afterwards, the annual produce of pig-iron there, was under 8000 tons. In forty years after Carron Works were in operation, or about 1805, seven additional iron-works appear to have been built, and about this period the yearly make of iron in Scotland might be about 24,000 tons. Only one more iron-work appears to have been built in the succeeding twenty years; and, in 1825, we find the annual produce of iron increased to 29,200 tons, the number of furnaces then built being twenty-five, of which only seventeen were in blast.

At the beginning of the current year, there were thirty-seven furnaces at work, producing at the rate of 99,500 tons of pig-iron annually, being an increase of twelve furnaces in twelve years, and an additional yearly make of iron of 69,000 tons, compared with 1825. Besides this increased number of furnaces in operation in 1837, there were five additional furnaces in progress of building, and four more contemplated, which, when completed, will produce 27,000 tons more iron yearly; the annual make of iron in Scotland, would thus be 126,500 tons, or about *one-sixth* part of the gross estimated make of iron in Great Britain yearly. In 1825, Scotland only produced about *one-twentieth* part of the make of iron in the United Kingdom.

This extraordinary increase in the iron trade of our country is principally found to have taken place in the county of Lanark, and the apparent physical causes which have contributed to the recent rapid extension of this trade in the Lower Ward of that county, may be considered as threefold.

1st. The moderate depth to, and thickness of, the seams of coal, which enables them to be readily and cheaply produced.

2nd. The abundant supply of what is locally called blackband ironstone, which, being of a bituminous nature, is easily calcined; and 3dly, the late discovery, and now general application at the iron-works of that county, of Neilson's hot blast, which has produced great economy in the quantity of coal requisite to produce good pig-iron; this improved mode of blast is also applied at all the other iron-works of Scotland. This blackband ironstone

is as yet chiefly peculiar to the parishes of Old and New Monkland, where it is found from a few inches to about two feet thick: and it is remarkable, that though about forty years have elapsed since its discovery by Mr. David Mushet (who, in the opinion of a late eminent iron-master, deserved a crown of gold for the discovery,) its general application in iron-making, is but of recent date. The importance attached to this ironstone by the iron-masters in the west, was distinctly marked in the fall of last year, by their uniting to pay a yearly rent of £12,000 for liberty to work it in part of the estate of Airdrie, extending to about 300 acres, where it is of good quality, and possesses peculiar facilities for being worked; and this circumstance has induced many proprietors in the adjoining parishes and counties to make trials to ascertain if this valuable ironstone be extensively deposited in the coal-fields there; some of these trials have proved successful, while others have entirely failed.

The editor of the *Merthyr Guardian* makes the following remarks:—

"We understand the Blackband Ironstone therein mentioned, and on which the extent and prosperity of the iron trade in Scotland chiefly depend, has been found in North Staffordshire, and lately (to the extent of 500 acres) in the estates of Lord Lichfield, in South Staffordshire. Something analogous, as it respects its conformability and connexion with the coal measures, has also been found at Bellingsley, in Shropshire, above two feet in the cross, and containing forty per cent. of iron, with a calcareous, not a carboniferous base, and there appears to us no good reason why the same, or a similar formation, may not be found (upon proper search being made) to pervade the South Wales Coal Basin; at any rate, we consider that we discharge an important duty in thus drawing the attention of the iron masters of this district to the subject."

The following letter from Mr. Mushet has since appeared in the *Scotsman*, "and refers," says the Editor of that Journal, "to the interesting communication of Messrs. Bald and Geddes, on the iron trade of Scotland and the Blackband ironstone." The discoveries and experiments detailed in Mr. Mushet's letter, appear to us peculiarly valuable, and well deserving the attention of the iron masters of South Wales:—

Sir,—I have read, with a good deal of interest, in your excellent and highly talented newspaper of the first of July, a statement of Messrs. Bald and Geddes, on the past, present, and probable future state of the iron trade in Scotland; the prospective advantages of which seem to rest in a great measure upon the general extension of the Blackband ironstone in the coal districts, and of obtaining a plentiful supply, for smelting, of that easy fusible and highly valuable carboniferous (not bituminous) ironstone.

I may be excused for taking an interest in the development of the question, as I happened to be the person referred to in the above paper who first made the discovery, and dared to apply it to the purpose of smelting, about thirty-four years ago, amidst the sneers and pity of the iron veterans of those days, at my folly in attempting to make iron from wild coal—for this was the name by which the Blackband was known, previously to its application to iron-making by myself.

As this ironstone has now become an object of considerable national importance, perhaps you will allow me room to allude more particularly to the circumstances of its discovery. Thirty-six years ago, this summer, in attempting to cross the river Calder on foot, in the Old Monkland Parishes, I obtained nearly a dry footing upon what appeared to be a continuous

black pavement, from the upper edge of which I struck off a piece of substance resembling coal, which, upon a closer examination, was found interlaminated with yolks of brownish matter, about three-eights of an inch in thickness. These, when separated from the carboniferous schist, proved to be good ironstone, but so small in quantity, that the average of the mass did not exceed 13 per cent. Next day I returned to the black pavement, better prepared to ascertain its extent and thickness. On removing a portion of the entire bed, I found it to be fourteen inches in thickness, viz., three and a half of coaly schist with the before-mentioned yolks, and ten and a half of more compact and solid matter, but still far removed from the usual appearance of ironstone or iron ore—its specific gravity being about 2.15. It was soon ascertained that this portion, being the richest, contained thirty-three per cent.—the average of the whole bed thirty per cent., in the raw state; but being entirely carboniferous, it was found to lose fifty per cent. in roasting, which was, and no doubt still is, performed without the addition of any fuel, thereby forming an ore perfectly prepared for the furnace, containing from sixty to seventy per cent. of iron. In a short time from making this discovery, I had ascertained the existence of the Blackband in 2000 acres of land, including the Airdrie estate (for right of working in which, to a limited extent, £12,000 a year is now about to be given,) and determined it to be a regular member of the coal formation.

Before venturing it in the blast-furnace, my experiments were numerous and are still extant; and even at this distance of time ("*the crown of gold*" being still in abeyance) may be referred to with satisfaction, as descriptive of that diversity of quality, form, and structure, so peculiar to this ore in roasting; not the least singular of which is, the occasional production of thin sheets of malleable iron interposed between lamina of the carboniferous schist.

I may further remark, in reference to Messrs. Bald and Geddes's paper, above alluded to, that although the use of the Blackband ironstone at the blast furnaces in Scotland may not have been so general as might have been expected; yet I believe its application has been constant at the Calder furnaces, from the period of its first introduction, and also at the Clyde furnaces for ten or twelve years past.

The removal of prejudice, and the consequent march of intellect, are, however, always slow, as I have recently experienced upon this subject; for, with some knowledge of the nature and quality of the bar iron likely to result from the Blackband (particularly if smelted with hot-blast,) and with a view to meet or remove the real or apprehended difficulties, I wrote to a party concerned in the welfare of the manufacture four months ago, but so *slow is the process of inter-communication in this country*, that the reply to my letter has not yet reached me.—I am, &c.

Coleford, Gloucestershire, July 12. DAVID MUSHET.

Ibid.

Progress of Physical Science.

SIR J. F. HERSCHEL *on the theory of Volcanic Phenomena.*

Extracts from a letter from Sir John F. W. Herschel to C. Lyell, Esq., dated Fredhausen, Cape of Good Hope, 20th February, 1836.

The author commences by inquiring, whether it had ever occurred to Mr. Lyell to speculate on the probable effect of the transfer of pressure from

one part to another of the earth's surface by the degradation of existing, and the formation of new continents, on the fluid or semifluid matter beneath the outer crust? Supposing the whole to float on a sea of lava, the effect would merely be an almost infinitely minute flexure of the strata; but supposing the layer next below the crust to be partly solid and partly fluid, and composed of a mixture of fixed rock, liquid lava and other masses in various degrees of viscidity and mobility; great inequalities may subsist in the distribution of pressure, and the consequences may be local disruptions of the crust where weakest, and escape to the surface of lava, &c.

Referring to the phenomena of volcanos, Sir J. Herschel observes, that it has always been his greatest difficulty in geology to find a *primum mobile* for the volcano, taken as a general and not as a local phenomenon; and referring to the different theories given on the subject, which he considers insufficient, wanting in explicitness and as not going high enough in the inquiry or up to its true beginning, and also as giving in some respects a wrong notion of the process itself;—inquires, how came the gases which are evolved to be condensed?—why did they submit to be urged into liquefaction?—if they were not originally elastic, but have become so by subterranean heat—whence came the heat, and why did it come?—how came the pressure to be removed, or what caused the crack?

It seems clear that if the gases or aqueous vapour were once free at so high a degree of elasticity as is presumed, there exists no adequate cause for their confinement. We are forced therefore to admit that the elastic force has been superadded to them during their sojourn below by an accession of temperature.

Assuming a high central temperature, which many geologists admit, and with which all are familiar; the author agrees with Mr. Lyell's observations, that the ordinary repose of the surface argues a wonderful inertness in the interior, where in fact he conceives that everything is motionless; debarred therefore from the invasion of a circulating current or casual injection of intensely hot liquid matter from below, he conceives that the phenomena may be explained as follows:

Granting an equilibrium of temperature and pressure within the globe, the isothermal strata near the centre will be spherical; but where they approach the surface they will by degrees conform themselves to the configuration of the solid portion, that is, to the bottom of the sea and the surface of continents. If we suppose therefore a state of equilibrium, and that under the concave bottom of any great ocean the lines of equal temperature be parallel to its concavity; when this comes to be filled up by the deposition of matter brought down by rivers, &c., the formerly concave bottom may become horizontal or even convex, and the equilibrium of temperature will immediately be disturbed; because the form of a stratum of temperature depends essentially on the bounding surface of the solid above it, that form being one of the arbitrary functions which enter into its partial differential equation. The temperature, therefore, will immediately begin to migrate from below upwards, and the isothermal strata will gradually change their forms from the concave to the horizontal or convex form. The former bottom of the ocean will then (after the lapse of ages, and when a fresh state of equilibrium is attained) acquire a temperature corresponding to its then actual depth; while a point as deep below it, as itself is below the surface, will have acquired a much higher temperature, and may become actually melted, and this without any bodily transfer of matter in a liquid state from below. But if the temperature of this supposed deep stratum be already at the melting

point, then will this rise to the former bottom of the ocean and the strata become melted, *water included*, with which, from the circumstances of the case, they must be saturated.

If the process of deposition go on, until by accumulation of pressure on the bottom or sloping sides, some support gives way,—a piece of the solid crust breaks down and is plunged into the liquid below, and a crack takes place, extending upwards. Into this the liquid will rise by simple hydrostatic pressure. But as it gains height it is less pressed; and if it attain such a height that the ignited water can become steam, the joint specific gravity of the column is suddenly diminished and up comes a jet of mixed steam and lava; till so much has escaped that the deposited matter takes a fresh bearing, when the evacuation ceases and the crack becomes sealed up.

By taking this view of the process of heating from below, we have a strictly theoretical explanation of the effects of heat on newly deposited strata; and this, simply because the fact of a new stratum having been deposited, the conditions of the equilibrium of temperature become altered, and they draw the heat to them, or rather retain it in them in its transit outwards; the supply from the centre being supposed inexhaustible, and its temperature of course invariable.

As the greatest transfer of material to the bottom of the ocean is produced on the coast line by the action of the sea, while the quantity carried down by rivers from the surface of continents is comparatively trifling; hence therefore the greatest local accumulation of pressure is in the central area of deep seas, but the greatest local relief takes place along the abraded coast lines: here, therefore, according to this view should occur the chief volcanic vents.

In this view the effects of the removal of matter from above to below the sea, are, 1st. It produces a mechanical subversion of the equilibrium of pressure. 2d. It also, and by a different process, produces a subversion of the equilibrium of temperature. The last is the most important. It must be an *exceedingly slow process*, and will depend, 1st. On the depth of matter deposited; 2d. On the quantity of water retained by it under the great pressure; 3d. On the tenacity of the incumbent mass—whether the influx of caloric from below, which *must take place*, acting on that water, shall either heave up the whole mass as a continent; or shall *crack* it and escape as a submarine volcano; or shall be suppressed until the main weight of the continually accumulating mass breaks its lateral supports at or near the coast lines, and opens there a chain of volcanos.

Thus the circuit is kept up—the *primum mobile* is the degrading power of the sea and rains (both originating in the sun's action) above, and the inexhaustible supply of heat from the enormous resources below, always escaping at the surface, unless when repressed by an addition of fresh clothing at any particular part. In this view of the subject the tendency is outwards. Every continent deposited has a propensity to rise again, and the destructive principle is continually counterbalanced by a reorganizing principle from beneath. Nay, it may go further; there may be such a tendency in the globe to swell into froth at its surface, as may maintain its dimensions in spite of its expense of heat, and thus preserve the uniformity of its rotation on its axis.*

* On the subject of the views here enunciated by Sir John Herschel, see a notice of a paper by Mr. Babbage, Lond. and Edinb. Phil. Mag. vol. v, p. 213. Mr. Babbage has since published the substance of his paper in his work entitled "The Ninth Bridgewater Treatise," together with the extracts from Sir J. Herschel's letters, abstracted

An Extract of a letter from Sir John F. W. Herschel to R. I. Murchison, Esq., in explanation of the former, to C. Lyell, Esq., dated Fredhausen, 15th November, 1836.

In this letter the author recapitulates the views given in the foregoing abstract, stating that his views are not so much a theory as a pursuing into its consequences, according to admitted laws, of the hypothesis of a high central temperature; and his object to get a geological *primum mobile* in the natue of a *vera causa*, and to trace its working in a distinct and intelligible manner, so that in future, instead of saying as heretofore, "*Let heat* from below *invade* newly deposited strata, then they will expand, melt," &c., we shall commence a step higher and say, "*Let strata be deposited*, then, as a necessary consequence, and according to known regular and calculable laws, heat will gradually invade them from below and around; and according to its due degree of tensity at any assigned time will expand or melt them as the case may be," &c. The phenomena of earthquakes, volcanic explosions, &c., may arise, but if all goes on in quiet, the only consequences will be the obliteration of organic remains and lines of stratification, and the formation of new combinations of a chemical nature, &c.; in a word, the production of *metamorphic* or stratified primary rocks.

In the formation of these therefore there is nothing casual; all strata once buried deep enough, and due time allowed, must assume that state. None can escape; all records of former worlds must ultimately perish.

Lon. & Ed. Phil. Mag.

Agreement of Geology with Revelation.

Charles Babbage, Esq., Lucasian Professor of Mathematics of the University of Cambridge, has recently published a work, which is announced as a Ninth Bridgewater Treatise, and which will doubtless meet with a reception correspondent with the high reputation of the author. It contains an article on the supposed discrepancy between the facts of modern Geology and the account of the creation in the First chapter of Genesis. This subject has excited abundant discussion both in religious and philosophical Journals, and in essays, and treatises, especially devoted to it. It has been carefully and critically examined, as it well deserves to be, and the result of the investigation is evidently tending to the general conviction that there is no actual discord between the truths of scripture as contained in the Mosaic Revelation, and the indubitable records of Creation which are continally brought to light by geological researches. The evidence of the senses, confirmed by years of experience, in all parts of the world, no reasonable man will presume to dispute. The question then turns upon the right conclusions with respect to the age of the world; to which these archives of nature necessarily lead, and also upon the right interpretation of the sacred text. On these points the views of Prof. Babbage will be read with no little interest. We take the extract from a late number of the Edinburgh New Philosophical Journal. G.

Those, who, without the knowledge which enables them to form an opinion on the subject, feel any latent wish that this evidence (the records of

above. He observes, in reference to the similarity of Sir J. Herschel's views to those he had himself previously started, "I feel, that the almost perfect coincidence of his views with my own, gives additional support to the explanations I have offered; whilst the reader will perceive, from the different light in which my friend has viewed the subject, that we were both independently led to the same inferences by different courses of inquiry."—Edit.]

nature) should be overthrown, would do well to remember that geology also furnishes strong evidence in favour of the much more direct statement of Moses, as to the recent creation of Man. And although we must ever feel a certain degree of caution in admitting negative evidence as conclusive; yet, in the present instance, the multitude of fossil bones which have been discovered, and which, when examined by persons *duly qualified* for the task, have been uniformly pronounced to be those of various tribes of animals, and not those of the human race, undoubtedly affords strong corroborative evidence in confirmation of the Mosaic account.

In truth, the mass of evidence which combines to prove the great antiquity of the Earth, is so irresistible, and so unshaken by any opposing facts, that none but those who are alike incapable of observing the facts and of appreciating the reasoning, can for a moment conceive the present state of its surface to have been the result of only six thousand years of existence.

What, then, have those accomplished who have restricted the Mosaic account of creation to that diminutive period, which is, as it were, but a span in the duration of the Earth's existence, and who have imprudently rejected the testimony of the senses, when opposed to their philological criticisms? Undoubtedly, if they have succeeded in convincing either themselves or others, that one side of the question must be given up as untenable, those who are so convinced are bound to reject that which rests on testimony, not that which is supported by still existing facts. The very argument which Protestants have opposed to the doctrine of transubstantiation would, *if their view of the case were correct*, be equally irresistible against the book of Genesis.

But let us consider what would be the conclusion of every reasonable being in a parallel case; let us imagine a manuscript written three thousand years ago, and professing to be a revelation from the Deity, in which it was stated, that the colour of the paper of the very book now in the reader's hand is *black*, and that the colour of the ink in the characters which he is now reading is *white*,—with that reasonable doubt of his own individual faculties, which would become the inquirer into the truth of a statement said to be derived from so high an origin, he would ask of all those around him, whether, to their senses, the paper appeared to be *black*, and the ink to be *white*. If he found the senses of other individuals agree with his own, then he would undoubtedly pronounce the alleged revelation a forgery, and those who propounded it to be either deceived or deceivers. He would rightly impute the attempted deceit to moral turpitude, to the gross ignorance, or to the interested motives of the supporters of it; and he certainly would not commit the impiety of supposing the Deity to have wrought a miraculous change upon the senses of our whole species, and to demand their belief in a fact directly opposed to those senses; thus throwing doubt upon every conclusion of reason which related to external objects, and, amongst others, upon the very evidence by which the authenticity of that questionable manuscript was itself supported, and even of its very existence when before their eyes.

Thus, then, had those who attempt to show that the account of the creation in the book of Genesis is contradicted by the discoveries of modern science, succeeded, they would have destroyed the testimony of Moses, they would have uncanonized one portion of Scripture, and, by implication, have thrown doubt on the remainder. But minds which thus failed to trace out the necessary consequences of their own argument, were not likely to have laid very secure foundations for the basis on which it rested; and I

shall presently prove that the contradiction they have imagined can have no real existence, that, whilst the testimony of Moses remains unimpeached, we may also be permitted to confide in the testimony of our senses.

Before entering on the main argument it may be remarked, that the plainest and most natural view of the language employed by the sacred historian of the Earth is, that his expressions ought to be received by us in the sense they were understood by the people to whom he addressed himself. If, when speaking of the creation, instead of using the terms light and water, he had spoken of the former as a wave, and of the latter as the union of two invisible airs, he would assuredly have been perfectly unintelligible to his countrymen. At the distance of above three thousand years his writings would just have begun to be comprehended, and possibly three thousand years hence, those views may be as inapplicable to the then existing state of human knowledge, as they would have been when the first chapter of Genesis was written.

Those, however, who attempt to disprove the facts presented by observation, by placing them in opposition to revelation, have mistaken the very groundwork of the question. The revelation of Moses itself rests, and must necessarily rest, on *testimony*. Moses, the author of the oldest of the sacred books, lived about 1500 years before the Christian era, or about 3300 years ago. The oldest manuscripts of the Pentateuch at present known, appear to have been written about 900 years ago. These were copied from others of older date, and those again might probably, if their history were known, be traced up through a few transcripts to the original author; but no part of this is revelation; it is testimony. Although the matter which the book contains was revealed to Moses, the fact, that what we now receive as revelation is the same with that originally communicated, is entirely dependent on testimony. Admitting, however, the full weight of that evidence, corrobated as it is by the Samaritan version; nay, even supposing that we now possessed the identical autograph of the book of Genesis by the hand of its author, a most important question remains,—what means do we possess of translating it?

In similar cases we avail ourselves of the works of the immediate predecessors and of the contemporaries of the writer; but here we are acquainted with no work of any predecessor,—of no writing of any contemporary; and we do not possess the works of any writers in the same language, even during several succeeding centuries, if we except some few of the sacred books. How, then, is it possible to satisfy our minds of the minute shades of meaning of words, perhaps employed popularly; or, if they were employed in a stricter and more philosophical sense, where are the contemporary philosophical writings from which their accurate interpretation may be gained?

The extreme difficulty of such an inquiry will be made apparent by imagining a parallel case. Let us suppose all writings in the English, and, indeed, in all other languages previous to the time of Shakspeare, to have been destroyed,—let us imagine one manuscript of his plays to remain, but not a vestige of the works of any of his contemporaries; and, further, suppose the whole of the succeeding works of English literature to be annihilated nearly up to the present time. Under such circumstances, what would be our knowledge of Shakspeare? We should undoubtedly understand the general tenor and the plots of his plays. We should *read* the language of all his characters; and, viewing it generally, we might even be said to understand it. But how many words connected with the customs, habits, and manners of the time must, under such circumstances, necessari-

ly remain unknown to us! Still further, if any question arose, requiring for its solution a knowledge of the minuter shades of meaning of words now long obsolete, or of terms supposed to be used in a strict or philosophical sense, how completely unsatisfactory must our conclusions remain? Such I conceive to be the view which common sense bids us take of the interpretation of the book of Genesis. The language of the Hebrews, in times long subsequent to the date of that book, may not have so far changed as to prevent us from rightly understanding, generally, the history it narrates; but there appears to be no reasonable ground for venturing to pronounce with confidence on the minute shades of meaning of allied words, and, on such foundations, to support an argument opposed to the evidence of our senses.

I should have hesitated in offering these remarks respecting the right interpretation of the Mosaic account of the creation, had the argument depended on any acquaintance with the language in which the Sacred Volume is written, or on any refinements of criticism, had I possessed that knowledge; but, in estimating its validity, or in supplying a more cogent argument, I entreat the reader to consider well the difficulties which it is necessary to meet.

1st, The Church of England, if we may judge by the writings of those placed in authority, has hitherto considered it to have been expressly stated in the book of Genesis, that the Earth was created about six thousand years ago.

2dly, Those observers and philosophers who have spent their lives in the study of geology, have arrived at the conclusion, that there exists irresistible evidence that the date of the Earth's first formation is far anterior to the epoch supposed to be assigned to it by Moses; and it is now admitted by all competent persons, that the formation, even of those strata which are nearest the surface, must have occupied vast periods,—probably millions of years,—in arriving at their present state.

3dly, Many of the most distinguished members of the Church of England now distinctly and formally admit the fact of such a lengthened existence of the Earth which we inhabit, for it is so stated in the eighth *Bridgewater Treatise*, a work written by the Professor of Geology in the University of Oxford—himself holding an office of dignity in that church, and expressly appointed to write upon that subject by the Archbishop of Canterbury and the Bishop of London.

4thly, The Professor of Hebrew at the same university has proposed a new interpretation of those passages of the book of Genesis which were hitherto supposed to be adverse to the now admitted facts.*

Such being the present state of the case, it surely becomes a duty to require a very high degree of evidence before we again claim authority for the opinion, that the book of Genesis contains such a precise account of the work of creation, that we may venture to appeal to it as a refutation of observed facts. The history of the past errors of our Parent Church supplies us with a lesson of caution which ought not to be lost by its reformed successors. The fact, that the venerable Galileo was compelled publicly to deny, on bended knee, a truth of which he had the most convincing demonstration, remains as a beacon to all after time, and ought not to be without its influence on the inquiring minds of the present day.

If the explanation offered by the Professor of Hebrew be admitted, those who adhere to it must still have some misgivings as to the effect of new dis-

* V. Buckland's Bridgewater Treatise, p. p. 22—5.

coveries in nature causing continual occasion for amended translations of various texts; whereas, should the view which has been advocated in this chapter be found correct, instead of fearing that the future progress of science may raise additional difficulties in the way of revealed religion, we are at once relieved from all doubt on that subject.—*Babbage's Bridgewater Treatise*, p. 63.

Ed. New Philos. Jour.

The November-Asteroïds.

Notice of a recent Communication relating to them, by Dr. Olbers.

Those who take an interest in physical sciences have, within these few years, had their attention directed to the periodical return of shooting-stars; and recent observations go far to subvert the opinions formerly held with respect to them—namely, that they originated within the limits of our atmosphere. On the contrary, it is now looked upon as an established fact, that they are heavenly bodies of inconsiderable dimensions, but which, in common with all others of a similar nature, have, of course, a regular motion. They are, in short, supposed to represent, in the solar system, the animacules of the animal kingdom.

Olbers, in the *Jahrbuch fur* 1837, brings forward, with irresistible force and clearness, numerous proofs in favour of this supposition; and the following general sketch will, it is hoped, give a faithful abstract of the arguments to which he therein has recourse, in favour of the new theory.

On the 12th of November, 1799, before sun-rise, Humboldt and Bonpland, on the coast of Mexico, observed, during four hours, a succession of many thousand shooting-stars and small fire-balls. The part of the heavens whence they emanated lay due east, and they extended over a space of 30° to the right and left of that point. They rose above the horizon in an east-north-east direction, and fell towards the south, describing unequal arcs in their course. Some attained an altitude of 40°, and there were none that did not rise to between 25° and 30°. Several appeared to burst; the largest, however, disappeared, without throwing out any coruscations. Some had a large nucleus equal to Jupiter in brightness, and all, or nearly so, had tails.

This remarkable phenomenon was also simultaneously observed from various distant points of the earth. A similar display was again witnessed on the night of the 13th of November, 1831, on the night of the 12th of November, 1832, on the same night in 1833, and on the night of the 13th of November, 1834.

This periodical return induced many persons, where the cloudy state of the weather did not altogether put a stop to their observations, to keep a good look out on the nights between the 11th and 14th of November, 1836; nor was the trouble they thus took thrown away, for it appears that a vast number of shooting-stars were actually at that time observed from the most distant points of the globe.

The periodical return of these bodies would thus seem to be established, but this is not sufficient for us to form any just notions as to their nature. The first step to be taken to that end is to discover what their elevation above the earth's surface may be.

In order to arrive at this, two observers in concert determine the angle under which they see a shooting-star, indicating likewise the time of its appearance, and noting the star near which it came into sight. Now, if two such observers remark at the same instant, and in the same direction and

position, any shooting-star, it may fairly enough be assumed that they are looking at one and the same object. A triangle is thus obtained; the base—that is to say the distance between the observers, and two angles of which, are approximately known, so that the calculation of the distance presents no difficulties whatever.

Simple as these observations are, it is evident that they cannot pretend to any thing like exactness; as they are, however, very numerous, their errors are thereby eliminated, and approximate results, at least, are arrived at; and the distance being thus ascertained, their real velocity may be deduced from the apparent.

Olbers conveys his opinions respecting these bodies in the following terms. The remarkable and very unequal distribution of those small bodies revolving round the sun, which form the fire-balls and shooting-stars of our planetary system, as well as the general resemblance, and nearly similar nature, of all the meteoric stones which fall from time to time, (taking into consideration both their external characters, and also their chemical composition,) seem to indicate not only that they have one common origin, but also that it was one common cause which has thus hurled them into space.

One cannot help reverting, involuntarily, to the hypothesis which holds the four new planets, Vesta, Juno, Pallas, and Ceres, to be but fragments of a larger planet, formerly revolving round the sun between Mars and Jupiter, but which has been shattered to pieces by some violent catastrophe. On such a planet as this exploding, and being scattered about in all directions there would also be, independent of the larger fragments, innumerable much smaller, indeed minute pieces, hurled into space, and these would now circulate about the sun in ellipses of various degrees of eccentricity.

It must be borne in mind that Olbers treats this, however, as a mere hypothesis and distinctly says he by no means adopts the language of Professor Wildt, who, as may be seen by referring to the 9th Volume, p. 408, of *Voigt's Magazin fur den neusten Stand der Naturkunde*, unreservedly asserts, the stones which have fallen on the earth to be "the ruins of some globe which has been destoyed, and which revolve round the sun till, sooner or later, they fall in with a planet.

"They without doubt, he continues, belong to the group of Ceres, Pallas, &c., and thus we see how it is that their appearance and composition bear such general resemblance to each other."

What we really know of shooting-stars, of those, at least, which are allied to fire-balls, if not actually identical with them, may be summed up in the following words:—

1st. They move at considerable elevations above the earth, their distances from it ranging from between 140 to 190 miles.

2ndly. Their velocity is equal to that of several of the planets; and their relative velocity, compared with the earth, may vary from 37 to 42 miles in a second.*

3rdly. Hence it is from without the regions of space that they enter our atmosphere, and it is not within the limits of the latter that they originally take their rise.

4thly. They are not ejected into the earth from the moon.

Much that is connected with these interesting meteors, it is, however, quite out of our power to explain. For instance, how is it they become ignited? How is it that, in the extremely rare medium in which they move,

* Littrow gives the mean velocity of Mercury only at 30 miles per second, that of the earth at 19 miles, and the mean of the four new planets at 12.

they continue glowing, or, indeed, burning with such brilliancy? How can the comparatively small mass of the meteoric stones which falls to the earth expand in those elevated regions to bodies several hundred feet in diameter?

Must we not suppose, with the acute Von Hoff, of Gotha, that the materials of which shooting-stars originally consist are submitted to a peculiar chemical process upon entering into our atmosphere, the result of which is the formation of the substance we see fall as meteoric stones,—a process which surely cannot consist merely of fusion?

All these questions, and many others of a similar nature, will possibly remain for ever unanswered, or be, at least, but unsatisfactorily solved; for it scarcely appears probable that time can give us a deeper insight into the circumstances connected with this subject. How can we, indeed, even hope to arrive at a satisfactory knowledge of processes carried on at altitudes where the air is far rarer than in the most perfect vacuum we can produce, and where, moreover, the atmosphere possibly consists of gases of which we know nothing, and all carried on at a temperature of absolute cold. What powers, affinity, electricity, and magnetism are there invested with, will always remain a mystery to us.

Ω.

Munich, 10th July, 1837.

Mag. Pop. Sci.

St. Elmo's Fire seen in Orkney.

During last February, 1837. (Sunday 19.) in a tremendous gale, my large boat sunk, and it was late on Tuesday night before we could get her up and drawn to the shore, after which we had to wait till three o'clock next morning till the tide ebbed from her; she was during this time attached to the shore by an iron chain, about 30 fathoms long, which did not touch the water, when, to my astonishment, I beheld a sheet of blood-red flame, extending along the shore for about 30 fathoms broad and 100 fathoms long, commencing at the chain and stretching along the shore and sea in the direction of the shore which was E. S. E., the wind being N. N. W. at the time. The flame remained about ten seconds, and occurred four times in about two minutes. Whilst I was wondering not a little, the boatmen, who, to the number of twenty-five or thirty, were sheltering themselves from the weather, came running down apparently alarmed, and asked me if I had ever seen anything like this before. I was about to reply, when I observed their eyes directed upwards, and found they were attracted by a most splendid appearance at the boat. The whole mast was illuminated, and from the iron spike at the summit, a flame of one foot long was pointed to the N. N. W., from which a thunder-cloud was rapidly coming. The cloud approached, which was accompanied by thunder and hail; the flame increased and followed the course of the cloud till it was immediately above, when it arrived at the length of nearly three feet, after which it rapidly diminished, still pointing to the cloud, as it was borne rapidly on to S. S. E. The whole lasted about four minutes, and had a most splendid appearance. I regretted afterwards that I was so occupied with the flame at the mast head that I did not observe whether the red flame on the ground continued during the time the cloud was passing.—*Extract of a Letter from William Traill, Esq., Kirkwall, to Professor Traill, dated May* 16, 1837.

Ed. New Philos. Jour.

Simultaneous Meteorology.—No. II.

Table of Hourly Meteorological Observations, made during the 21st and 22nd June, 1837, at Blackheath Road, near Greenwich, about four miles and a half S. E. of London, *by and under the superintendence* of J. H. Belville, in conformity with the instructions circulated by the South African Literary and Philosophical Institution.

DATE.	HOUR.	BAROMETER.	THERM. Attached.	THERM. In open air.	WIND. Direction.	WIND. Strength.	Pro. of cloud.	STATE OF THE ATMOSPHERE, CLOUDS, &c.
		Eng. In.	Fr. °	Fahr. °				
Wednesday, June 1837.	VI. A.M.	30.028	67	57.1	W.	1	10	*Stratus.* The *minimum* of the self-registering thermometer before sunrise, 54.5°.
	VII.	30.046	67	59.0	W.	1	10	Ditto.
	VIII.	30.052	68	62.8	W.	1	5	*Stratus* into *cumulus.*
	IX.	30.054	68	67.8	W. by S.	1	5	*Cirro-cumulus.* Hazy.
	X.	30.075	69	69.0	W. by S.	1	4	*Cumulus.*
	XI.	30.085	70	67.0	S.W.	1	3	Ditto.
	XII.	30.085	71	69 9	S.W.	2	3	Ditto. Brisk wind and cool.
	I. P.M.	30.098	72	70.5	S.W.	2	3	Ditto. Ditto ditto.
	II.	30.098	73	73.4	W.S.W.	1	6	Ditto.
	III.	30.100	73	73.6	W.S.W.	1	7	The same as before.
	IV.	30.103	72	71.2	W.S.W.	1	8	Dense *cumulo-stratus* has prevailed since last observation.
	V.	30.110	71	71.1	W.	2	5	Getting clearer and lighter.
	VI.	30.134	72	69.1	W. by S.	2	5	Getting more clear.
	VII.	30.150	74	68.2	S.S.W.	1	6	*Cirro-cumulus* to the S.
	VIII.	30.164	73	66.5	S.W.	1	8	Ditto ditto collecting. [&W.
	IX.	30.176	72	62.9	S.W.	1	1	Getting clearer again towards the N.
	X.	30.198	71	60.6	W.S.W.	1	4	Clouds again collecting to N. of zenith
	XI.	30.200	71	59.5	W.S.W.	0	4	Nearly cloudless. *Moon rises in a clear horizon.*
	XII.	30.205	71	58.0		0	1	Cloudless.
Thursday, June 22.	I. A.M.	30.210	71	54.5		0	0	Ditto. Calm and serene throughout the night.
	II.	30.231	71	53.6		0	0	Do. A low creeping mist on the distant hills to the S. a little before sunrise. The *maximum* of the self-registering thermometer at sunrise, 50.0°.
	III.	30.240	71	53.0	none	0	0	Do.
	IV.	30.246	70	51.7		0	0	Do.
	V.	30.250	70	54.0	W.	0	0	Do.
	VI.	30.262	69	55.8	W.	1	0	Do.
	VII.	30.281	69	59.0	S.S.W.	0	2	Light *cirai.* A breeze coming on.
	VIII.	30.293	68	63.5	N.W.	0	3	Great obscurity from mist and smoke.
	IX.	30.296	68	66.0	N.	1	1	Wind changes to N. Mist increases.
	X.	30.298	69	70.0	N.	1	1	A few *cumuli* appear through the mist.
	XI.	30.300	70	71.5	S.W.	1	2	So misty that the sun scarcely produces a shadow.
	XII.	30.301	71	73.0	W.	1	3	*Cumuli* & *cirro-cumili.* Mist and smoke still prevail.
	I. P.M.	30.301	71	69.0	N.	1	4	Ditto.
	II.	30.302	72	72.0	N E.	1	3	Ditto.
	III.	30.300	72	71.0	N.E.	0	4	Ditto.
	IV.	30.300	72	71 3	E.	0	5	Clouds and mistiness rather going off.
	V.	30.298	73	71 2	E.	0	4	A little clearer.
	VI.	30.300	72	69.8	E.	0	4	No variation since the last observ.

Notes.—The two days rather cloudy and misty; the intervening night clear and serene. The whole of the clouds came up from the *westward.* The *mist* prevailed chiefly in the *lower* atmosphere.

A little before midnight, a large fire broke out to the N. W. between Deptford and

Progress of Civil Engineering.

Telegraphic Communication betwixt London and Edinburgh.

From the Scotsman.

It has been found by experiments made with a view to ascertaining the velocity of electricity, that it is transmitted instantaneously, by means of a common iron wire, a distance of eight miles; and electricians of the first eminence have declared their opinion that, judging from all scientific experience, the electric or galvanic influence would be almost instantaneously transmitted from one end to the other of a metallic conductor, such as ordinary copper wire of moderate thickness, of some hundred miles in length.

If this scientific theory is correct, it follows that a wire, secured by a coating of non-conductors, and protected from external influence or injury, and laid under the turnpike-road between Edinburgh and London, could be the means of distinctly indicating to a person stationed in London, that such wire had been electrified or galvanized in Edinburgh—the transmission of the electric or galvanic influence being clearly discernible by various well known means.

How, then, is this scientific fact to be applied to purposes of practical and general utility? Simply by laying as many wires separated from each other as will correspond to the letters of the alphabet, and preconcerting between the persons stationed at two extremities of the line of communication, that each individual wire is to represent a particular letter; because, if the person stationed in Edinburgh can, by applying the electric influence to any one wire, instantaneously apprize another person stationed in London that a particular letter of the alphabet is thereby indicated, words and sentences *ad infinitum*, may be communicated, and the idea of a perfect telegraph would be realized.

Without experience it is impossible to say with what rapidity this electro-magnetic telegraph could be worked: but, in all probability, intelligence could be conveyed by such a medium as quickly as it is possible to write, or at least to print; an apparatus could be constructed somewhat resembling the keys of an organ, by which the letters of the telegraph could be touched with the most perfect ease and regularity.

It has been mentioned, that the transmission of the electricity or galvanism could be discernible by various means well known. If any indication,

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Rotherhithe, at a spot about two miles in a direct line from the Meteorological Station, from which a dense column of smoke rose nearly perpendicular to a great elevation. *No perceptible current of air could be detected.* The night ended remarkably still and calm.

The time was taken from a good clock, keeping mean time, *rate* scarcely perceptible during the observations; *error* obtained by observation of the Ball-Drop, Royal Observatory, Greenwich.

The Barometer has an elevrtion of about 40 feet above the River Thames.—Makers, Watkins and Hill, London; reads off about .025 lower than a standard barometer lately made by the same artists. The Thermometer has a northern aspect—Maker, Dollond.

The *strength* of the wind is indicated by the figures, thus—0 means *no wind perceptible;* 1, *very light breeze;* 2, *strong breeze.*

In the column headed *Proportion of cloud,* 0 signifies *quite clear;* 10, *no blue sky visible;* 5, *sky half covered,* &c.

however slight, is made, that is enough, all that is wanted being that it should be perceivable by the person placed to watch the telegraph.

It has been assumed, that the electric current is capable of transmission by means of a single impulse from Edinburg to London. But it is not indispensible that so great a distance should be accomplished at once. Intermediate stations for supplying the telegraph with new galvanic influence could be resorted to, and its perfect efficiency still preserved.

The best mode of troughing or protecting the metallic conductors, and separating them both from each other, and from the surrounding substances by which the electric or galvanic influence might be diverted, would, of course, require considerable scientific and mechanical skill; but the object appears perfectly attainable. Insulating or non-conducting substances, as gumlac, sulphur, resin, baked wood, &c., are cheap; and the insulation might be accomplished in many ways. For example, by laying the wires, after coating them with some non-conducting substances, in layers betwixt thin slips of baked wood, similarly coated, the whole properly fastened together and coated externally. These slips might be perhaps ten yards long, and at the joinings precautions for the expansion and contraction of the wire by the change of temperature, might be adopted. The whole might be enclosed in a strong oblong trough of wood, coated within and pitched without, and buried two or three feet under the turnpike road.

The expense of making the telegraph proposed is, of course, an important element in the consideration of its practicability and utility.

The chief material necessary, viz., copper wire, is by no means expensive. It is sold at 1*s.* 6*d.* per pound, of sixty yards in length. The cost of a wire from Edinburg to London, say 400 miles, would thus be about 900*l.*; but say for solderings, &c., 100*l.* additional, or that each copper wire, laid from Edinburg to London, would cost 1,000*l.* sterling, and that the total expense for the wires necessary to indicate separately each letter of the alphabet, would be 25,000*l.* The purchase of so large a quantity would, of course, be made at a considerably less price; but probably one or two additional wires might be needed, and the circuit of the electrical influence must be provided for by one or more return wires.

The coating, separating, and troughing of the wires can be accomplished by low-priced materials, and the total expense of the whole work (except the price of the wires,) allowing a large sum for incidental expenditure, has been roughly estimated at 75,000*l.*, making a maximum expenditure of, say, 100,000*l.* for the completion of the telegraph. For a proportional additional sum it might be extended to Glasgow.

As to the working of the telegraph, it is apprehended, that even if the speed of writing were not attained, there could at least be no difficulty in indicating one letter per second. At this rate, a communication which would contain sixty-five words would occupy about five minutes. This is supposing the vowels to be all indicated. But abbreviation in this, and many other respects, would no doubt be contrived; and the number of words in the communication supposed, are greater than necessary for an ordinary banking or commercial letter, or for friendly inquiries and responses. Supposing, however, that each communication was to occupy five minutes, and to be charged five shillings each, if the telegraph was worked twelve hours a day (that is, six hours from each end,) it would produce a revenue of 36*l.* daily, or 10,800*l.* per annum, supposing there were 300 working days in the year. If, however, the plan is practicable, the public intelligence that

would, no doubt, be transmitted by the telegraph would be sufficient to keep it in operation night and day.

Arrangements are being made for having the necessary experiments tried on a metallic conductor of fifty or a hundred miles in length, and if the same instantaneous and perfect indication of the passage of the electric or galvanic fluid is found to take place, as in the case of the recent experiments at the University, the triumph of the scheme would be complete.

Mech. Mag.

Captain Ericsson's new Propeller.

The American packet ship "Toronto" of 630 tons burthen, and drawing 14 feet 6 inches water, was on Saturday last towed down the Thames at the rate of full 4½ knots an hour *against* wind and tide by an experimental steam-boat, called the "Francis B. Ogden," (after Mr. Ogden, the able and intelligent Consul of the United States, at Liverpool,) fitted with the new propelling apparatus lately invented and patented by Capt. Ericsson. The "Francis B. Ogden" measures 45 feet in length, 8 feet beam, and draws 2 feet 3 inches water without the keel. The propelling apparatus is placed at the stern, and works entirely under the water. It consists of a peculiar application of the old, and well known principle of the water screw, by which a great propelling power is concentrated in a small space. Of the degree of power concentrated, no better proof can be adduced, than the fact, that the speed of 4½ knots against wind and tide, was produced by an apparatus, measuring only 5 feet 2 inches in diameter, and 2 feet 2 inches wide, and worked by a high pressure engine having two cylinders of 14 inches stroke, and 12 inches diameter; and which, during the experiment, made only 60 strokes per minute, and showed a pressure of not more than 50lbs. to the square inch.

The new propelling apparatus consists of two short cylinders made of thin wrought iron, and supported by arms of a peculiar form, which are placed entirely under the water at the stern, and made to revolve in contrary directions round a common centre. To the outer periphery of each cylinder, there is attached a series of spiral planes or plates, which may, we understand, be placed at any desired angle according to the effect sought to be obtained, whether it be great speed, or great propelling power.

The apparatus may be made to ship and unship at pleasure; the engine that works it may also be loco-moveable, so as to be worked upon deck, and on any part of the deck, and in these two peculiarities, we are inclined to think the chief advantage of this new step in steam navigation will be found to consist. Sailing vessels may, by this means, command all the aid that steam can give them, without divesting themselves of any of their peculiar fitness for long sea voyages, or undergoing any change in their original construction.

We subjoin a copy with which we were favoured of the certificate given by the pilot and mate of the "Toronto" of the performance of the "Francis B. Ogden" on this occasion.

Packet Ship Toronto, in the Thames, 28th May, 1837.

We feel pleasure in certifying that your experimental steam boat, the "Francis B. Ogden," has this morning towed our ship at the rate of 4½ knots an hour, through the water and against the tide.

(Signed) E. Nashly, Pilot.
H. R. Hovey, Mate.

To Captain Ericsson.

Ibid.

Liverpool and Manchester Railway.

The cost of working the Liverpool Railway for the year 1836, (if the reports published by the directors be correct,) was, deducting interest paid on borrowed money, 133,225*l.* 7s., or at the rate of upwards of 420*l.* per working day. Of this sum 41,952*l.* 5s. 8d. is stated to be the cost of locomotive power for the thirty miles of railway on which locomotive engines are alone employed. The cost of locomotive power was, therefore, just 1400*l.* a mile.

While the facts above stated are startling to parties who believe in and promulgate the doctrines of the cheapness of railway formation and maintenance, what a light do they not throw on the profound wisdom of "Parliamentary private bill legislation?"

About 30,000*l.* was expended by the original promoters of the Liverpool Railway in order to prove to Parliament that it could be easily completed at a cost of 510,000*l.*, or about 16,000*l.* per mile, as stated in the first act of incorporation. Since that period nearly as much more has been expended in Parliament in obtaining new acts, which demonstrate that the railway will cost more than three times 16,000*l.* or 50,000*l.* sterling per mile, and that the original proof was an entire fallacy.

Nor is this instance a solitary one. About three years ago, the promoters of the London and Birmingham Railway were compelled to spend 50,000*l.* in proving to Parliament that it could be formed for 2,500,000*l.*, or about 22,000*l.* per mile. The London and Birmingham Railway Company are now before Parliament, proving it must cost 4,500,000*l.*, or about 40,000*l.* a mile, and this at a period when the undertaking is little more than half finished. In the course of a year or two this estimate will no doubt be raised to the Liverpool Railway standard of 50,000*l.*

To work this railway at Liverpool railway prices will cost about 500,000*l.* a year, or about 1600*l.* per day, of which 150,000*l.* will be for locomotive power alone.

It is needless to multiply examples—it is sufficient to say that many thousand pounds are now in progress of being expended every week before Parliament, in proving that lines of railway can be completed through difficult tracts of country at from a third to a fifth of the cost which experience has demonstrated as necessary to construct a perfect modern railway, and that when completed they can be worked and maintained for a half and a third of the cost per mile now expended on the Liverpool Railway.

Min. Jour.

Nothwithstanding this enormous cost and annual expenditure, we find by the last quotations of the price of stocks, that the Liverpool and Manchester Railway stock is 206*l.* for 100*l.* and that of the London and Birmingham, 130*l.* for 85*l.* paid. The former, (it is stated in a late number of the Liverpool Times,) is now paying ten per cent, and has never paid less than eight. The latter is computed, in consequence of a greater thoroughfare, to yield a clear profit of at least thirteen per cent on the original shares.

G.

Health of Tunnels.

Report on the Primrose-hill Tunnel on the London and Birmingham Railway, by John Paris, M. D., Thos. Watson, M. D., Cantab., Wm. Lawrence, Esq., and R. Phillips, Esq.

We the undersigned, visited together, on the 20th February, 1837, the Tunnel now in progress under Primrose-hill, with the view of ascertaining

the probable effect of such Tunnels upon the health and feelings of those who may traverse them.

The Tunnel is carried through clay, and is lined with brickwork. Its dimensions, as described to us, are as follows: height 22 feet, width 22 feet, length 3,750 feet. It is ventilated by five shafts, from 6 to 8 feet in diameter, their depth being 35 to 55 feet.

The experiment was made under unfavourable circumstances. The western extremity of the Tunnel being only partially open, the ventilation is less perfect than it will be when the work is completed. The steam of the locomotive engine also was suffered to escape for twenty minutes, while the carriages were stationary near the end of the Tunnel; even during our stay near the unfinished end of the Tunnel, where the engine remained stationary, although the cloud caused by the steam was visible near the roof, the air for many feet above our heads remained clear, and apparently unaffected by steam or effluvia of any kind; neither was there any damp or cold perceptible.

We found the atmosphere of the Tunnel dry, and of an agreeable temperature, and free from smell or perceptible effluvia of any kind; the lamps of the carriages were lighted; and, in our transit inwards and back again to the mouth of the Tunnel, the sensation experienced was precisely that of travelling in a coach by night, between the walls of a narrow street. The noise did not prevent easy conversation, nor appear to be much greater in the Tunnel than in the open air.

Judging from this experiment, and knowing the ease and certainty with which thorough ventilation may be effected, we are decidedly of opinion that the dangers incurred in passing through well-constructed Tunnels are no greater than those incurred in ordinary travelling upon an open railway or upon a turnpike-road; and that the apprehensions which have been expressed that such Tunnels are likely to prove detrimental to the health, or inconvenient to the feelings of those who may go through them, are perfectly futile and groundless.

John Paris, M. D.

Thomas Watson, M. D., Cantab., Physician to the Middlesex Hospital, and Professor of Medicine at King's College.

William Lawrence, Surgeon to St. Bartholomew's Hospital.

Rd. Phillips, Lecturer on Chemistry at St. Thomas's Hospital.

London, Feb. 21, 1837.

Mech. Mag.

Russian Column.

A Column has recently been erected to the memory of Alexander 1. in Petersburg, in the Place ——, of which the following are the dimensions:— Shaft of the pillar, in one piece of granite, length, 84 feet; diameter, 12½ feet; weight 350 or 400 tons. The bronze figure on the top is 15 feet high, independent of the cross, or 23 feet with the cross; weight, 13 tons. The bas-reliefs are in four plates, and the length of each 17½ feet; the height being 10½ feet. Some of the figures stand out nearly 2 feet. The weight of the four plates is 44 tons; entire height of the monument, 154 feet; weight of brass, 158 tons. The erection of this monument cost 10,000,000 of rubles, or £400,000 sterling; and it was erected in two years and a half. The stone pedestal on which the whole stands is 28 feet square, and 7 feet hick.

Architec. Mag.

Mechanics' Register.

English Patent-Law Grievance.

The inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the government during the past year, above £42,000. *What did the attorney-general, &c., in return to them for this vast and oppressive extortion?*

If we look a little further back, the following exhibition of this legal wrong will then present itself.

TOTAL SUMS paid by Inventors, in Ten Years, ending December 1834, according to the Returns made, and calculations of extras and occasional charges.

	£.	s.	d.		
English patents per report	106	11	8		
Stamp duty on specification, average	8	0	0		
Official charge of drawing, do. do.	5	5	0		
Extras, joint names, private seals, journeys, expedition caveats, attendance in opposition, &c. &c., *exclusive* of drawings and extra charges of the patent solicitor	12	3	4		
	132	0	0	× 1,684 Patents	= 222,288 £
Stamp Duty on Warrants, Colonies	6	0	0	× 1,684	= 10,104
Scotch patents, per report	79	10	5		
Stamp duty on specifications, average	8	0	0		
Charge of drawing, do. do.	5	5	0		
Extras, joint names, &c. &c.	7	4	7		
	100	0	0	× 489	= 48,900
Irish Patents, per report	128	5	11		
Stamp duty on specification, average	4	0	0		
Drawing, do. do.	5	5	0		
Extras, joint names, &c. &c.	7	9	1		
	145	0	0	× 207	= 30,015
Colonies, at additional charge with extras, average	7	10	0	× 300	= 2,240
Total amount paid by inventors in ten years, on 2380 patents					£313,557

MACKINNON. *Speech in the House of Commons, Feb.* 14, 1837, *Appendix.*

Three hundred and thirteen thousand, five hundred and fifty-seven pounds, "paid by inventors in ten years," *exclusive,* it will be observed, of the drawings and charges of the patent-solicitors on 2380 patents.

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult, in the shape of government stamps and fees on patents, amount to more than £11,000!

N. B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives,*) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c. Mag. Pop. Sci.

Preservation of Mineral Specimens from decomposition.

Several questions have been brought forward, as to the best method of preserving from decomposition fossil fruits, wood, &c.; and various methods

have been proposed with a view of attaining that object. It has been suggested, that after washing with turpentine and lamp-oil, a thin coating of isinglass in solution may be advantageously employed. On the 20th of last September, a friend brought me some fossils from Sheppey in pyrites, of which one specimen in particular was more affected by decomposition than the others. I immediately, after cleaning, gave them a dressing of turpentine and lamp-oil, recommended by Mr. Hill, and laid them in the open air to dry; but, in only two days after this, I found decomposition beginning afresh, the sulphate of iron forming upon them. I removed them into the house, wiped the lamp-oil clean off, and then gave them a coating of dissolved isingless and spirits of wine; and, at the end of seven months after this simple process, I find that they are perfectly free from the slightest appearance of decomposition; and have every reason to conclude that, for the purposes of the cabinet at least, this process for their preservation will be found sufficient. On applying the lamp oil, I found it of little or no use in this instance: it will not dry, it fills up the fibrous appearance on the outside of the fossils, and almost destroys their beauty. The quality of the isinglass obviates this completely; and it dries almost instantaneously.

Mag. Nat. His.

Fossil Infusoria.

Fossil infusoria have been eaten in Lapland in times of great scarcity. M. Retzius, Professor of Anatomy at Stockholm, has recently written thus to M. Ehrenberg:—"Through the kindness of M. Berzelius, I have received some fragments of the siliceous deposit of Franzensbad, sent by you to him. The carapaxes of the fossil infusoria contained in this deposit, made me think of a mineral substance vulgarly called Bergmehl (flour of the mountains,) analyzed and described by M. Berzelius in the Annales de Poggendort, for the year 1733. This flour of the mountains contains silex, an animal substance, and the *crénique* acid *(quellen saure,)* discovered by this great chemist. The Laplanders mix the bergmehl, when a famine takes place, with the flour of corn and bark, in order to make bread. They fed in this manner in 1833, in the little district of Degerfors, on the frontiers of Lapland, in the sixty-fourth and sixty-fifth degrees of latitude. On examing the mountain flour, which is considered by these superstitious people to be a gift from the great spirit of the forests, I have discovered nineteen different forms of infusoria, with siliceous carapaxes, of which I send you drawings. The whole of the mineral is composed of them, and the conjecture which I formed on its analogy with the deposit at Franzensbad, proves to have been well founded. M. Ehrenberg has received the mineral flour from Lapland and several of the infusoria which it contains are supposed to be still living near Berlin. The infusoria are *eaten* in Degerfors—I do not say that the inhabitants are nourished by them."—*Baron de Humboldt.*

Min. Jour.

The endless Ladder.

A patent has recently been obtained for a most ingenious and useful machine, adapted to mining and many other purposes, where the main object is to raise or lower weights and packages in constant succession. This simple, but very effectual contrivance, consists of an endless ladder, made

either of chain or rope, which passes *over* and *under* two revolving drums or cylinders, mounted upon horizontal axes; one placed at the bottom, and the other at the top, of a shaft or plain, to or from which the ladder is intended to reach. A continuous motion being given to either of the cylinders by the power of steam, or animal force, the endless ropes or chains, furnished with horizontal staves, like those of a common ladder, are made to circulate over the revolving cylinders by which they are extended, so that one part of this endless ladder is continually ascending with a slow but uniform motion from the lowermost of the cylinders to the uppermost, whilst *vice versa*, the other part of the ladder is descending to the lowermost in an uninterrupted circulation. A vast deal of labour is thus unremittingly performed, with the important result of great economy in time and power. The invention also provides a safe and easy conveyance for men; the accomplishment of which, in a philanthrophic, as well as any other point of view, has long been a desideratum in mining operations. For this purpose, a small movable step or footboard, furnished with a handrail, is applied, which, if desired, can be made wide enough to admit of several persons standing abreast, who are, by this means, passed up and down without fatigue, and in perfect security. Independently of the certain advantages that would result from the application of such machinery to the purposes for which it appears so admirably adapted, we consider Dr. Spurgin, (of London, the inventor of this apparatns.) to have thus planned a most admirable contrivance for the poor miners, a numerous class of our fellow citizens, who, from the peculiar nature of their occupation, are exposed to fearful risks of life and limb, and whose casualities would be materially diminished by the adoption of this machine. Min. Jour.

Simultaneous Magnetic Observations.

In accordance with the proposition of Prof. Gauss, simultaneous magnetic observations are now made on fixed days, at various places from Upsal to Palermo, of which Munich is the connecting point between the north and south, and, from the vicinity of the Alpine ranges, one of considerable interest. By the kind assistance of several of his friends and colleagues, Prof. Steinheil has been enabled to make regular observations during the two last of these "magnetic periods" of 24 hours each,—that is to say, on the 28th of November 1835, and the 30th of January 1836; the results had been communicated to Prof. Gauss by whom they would be laid before the public. He then showed the section a copy of observations thus made to Göttingen and Munich, graphically reduced; when thus compared, the similarity of the two curves was very remarkable, and evidently must have proceeded from causes operating simultaneously. Mag. Pop. Sci.

Animalculæ.

In the last published part of the new edition of the "Enclopædia Britannica," under the article microscope, by Sir David Brewster, we have the astounding information that "the size of a single individual of the animalculæ lately discovered in silicious rocks is 1-288th of a line, or 1-3456th of an inch. In the polishing slate from Bilin, in which there appear to be no vacuities, a cubic line contains, in round numbers, 23,000,000 of these animals, and a cubic inch contains 41,000 millions of them! The weight of a cubic inch of the polishing slate is 270 grains. There are, therefore, 187 millions of these animals in a single grain, or the silicious coat of one of these animals weighs the 187-millionth part of a grain." Min. Jour.

LUNAR OCCULTATIONS FOR PHILADELPHIA, JANUARY 1838.

Angles reckoned to the right or westward round the circle, as seen in an inverting telescope.
☞ For direct vision add 180° ☜

Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
5	4	48	Im.	♂ Arietis	4,	78°	25°
5	5	40	Em.			349	301
5	11	20	Im.	T' Arietis,	6,	124	177
5	12	31	Em.			273	327
9	14	34	N.	App. ☽ & 47Gem. ☽ N.l.'l	6,		
10	10	9	Im.	'ω Cancri	6,	74	18
10	11	37	Em.			263	227
10	11	37	N.	App. ☽ & ω² Cancri ☽ N. l.'l			

Meteorological Observations for August, 1837.

Moon.	Days.	Therm. Sun rise.	Therm. 2 P.M.	Barometer. Sun rise.	Barometer. 2 P.M.	Wind. Direction.	Wind. Force.	Water fallen in rain.	State of the weather, and Remarks.
				Inches	Inches			Inches.	
⊗	1	67°	83°	29.80	29.85	W.	Moderate.		Clear—do.
	2	67	85	84	82	W.	do.		Clear—do.
	3	74	88	78	60	W.	Brisk.	.45	Clear—do.—shower.
	4	63	73	77	80	NW.	Moderate		Cloudy—do.
	5	57	74	95	30.04	W.	do.		Clear—do.
	6	56	78	30.23	29	W.	Calm.		Clear—do
	7	63	76	15	10	E.W	do.	.26	Drizzle—cloudy.
	8	70	83	00	29.92	W.	Breeze		Clear—do.
☾	9	74	84	29.70	64	W.	Mod rate	.25	Cloudy—shower.
	10	72	78	64	66	NW.	do.		Clear—cloudy.
	11	69	74	75	81	E	Brisk.	.19	Cloudy—rain.
	12	61	64	83	90	NE.	M derate	.20	Rain—cloudy.
	13	64	73	30.00	30.00	NE W.	Calm.		Cloudy—do.
	14	65	75	0	29.93	SW.	Moderate.		Fog—cloudy.
	15	63	74	29.98	96	E.SW.	do.		Cloudy—clear.
☺	16	64	73	91	91	E.SW.	do.		Cloudy—clear.
	17	65	76	90	85	SE.	do.		Fog—cloudy.
	18	69	82	85	85	SE.	do.		Cloudy—Clear.
	19	70	81	85	85	E.	do.		Lightly cloudy—do. do.
	20	68	72	75	72	E.	Blustering.		Cloudy—do.
☽	21	63	80	62	62	NW.W.	Moderate.		Clear—do.
	22	68	72	53	55	NW.	Brisk.		Clear—do.
	23	56	66	75	75	W.	do.		Clear—partially cloudy.
	24	57	69	80	80	W.SE	Moderate	.5	Partially cloudy—do. do.—rain.
	25	54	73	80	90	W.	do.		Clear—do.
	26	56	71	90	86	SE.	do.	.56	Clear—cloudy—rain.
	27	68	77	75	74	W.	do.		Cloudy—clear.
	28	58	69	80	80	W.	Brisk.		Clear—do.
	29	57	76	86	86	W.	Moderate.		Clear—do.
⊗	30	68	86	66	66	W.	Brisk.		Clear—do.
	31	66	69	70	75	NE.	Moderate		Cloudy—rain.
	Mean	64.26	75 61	29 83	29.83			2 06	

	Thermometer.	Barometer.
Maximum height during the month	88.00 on 3d.	30.29 on 6th.
Minimum do.	54.00 on 25th	29.53 on 22nd
Mean do.	69.95	29.83.